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PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*Notes in Practical Surgery.* By GEORGE H. PORTER, M.D., T.C.D.; F.R.C.S.I.; Senior Surgeon to the Meath Hospital and County of Dublin Infirmary; Examiner in Surgery Royal College of Surgeons, Ireland; Consulting Surgeon to the Coombe Lying-in Hospital; Member of Council of the Surgical Society of Ireland; Member of the Pathological Society of Dublin; Lecturer on Clinical Surgery.

1. FOUR SUCCESSFUL CASES OF STONE IN THE URINARY BLADDER REMOVED BY THE OPERATION OF LITHOTOMY.
2. ANEURISM OF THE POPLITEAL ARTERY CURED BY DIGITAL PRESSURE IN TWENTY-SIX HOURS AND FORTY-FIVE MINUTES.
3. CASTRATION; THE VESSELS OF THE CORD SECURED BY ACUPRESSURE.

I HAVE often regretted that the valuable Dublin Hospital Reports have not been continued to the present time in the same able manner in which they were written and published by distinguished members of our profession who are now at rest. I believe that detailed

accounts of cases of operative surgery brought to a successful termination, with a record of the various stages of progress, are invaluable, and will always be perused with interest by the earnest surgeon. It will be admitted also that the professional man who is placed under circumstances which afford opportunities of performing difficult and uncommon operations, is called upon to extend to others whatever experience he acquires. Under this conviction, I have selected from my note book the following cases. Those on lithotomy I consider most noteworthy, because among the diseases to which man is liable there are few which present themselves to the surgeon with more interest than calculus in the bladder. This is not to be wondered at when we take into consideration the extreme sufferings of the patient, which are so frequent and acute as almost to make him wish for death to relieve him. When we remember, moreover, the uncertainty which prevails as to the constituent parts of the stone, and the consequent impossibility of rationally administering medicines with a view to its solution; or when we bear in mind the severe and dangerous operation to which sooner or later the patient is obliged to submit as his only remedy. And as to the operation I may add that, whether we consider its early history, the various modes of performing it, the average of fatal cases in a given number, the dangers attending it, the quickness and brilliancy of its performance, or the relief which it affords in an unerringly fatal disease, lithotomy is one of the most prominent operations in surgery, and gathers round it the largest possible amount of interest for the practical surgeon.

Case I. — *Man aged Thirty-eight years; Lateral Operation; Recovery.*—Thomas D., a farmer, from the County Kildare, was admitted into the Meath Hospital, under my care, Sept. 23rd, 1858. The following is the history of his case as related by himself. About two years previous to his application at the hospital he was suddenly attacked with severe pain in the loins, while engaged at his employment, and immediately afterwards noticed that he passed bloody urine. This subsided in a day or two, and he was then annoyed by pain at the side of his glans penis, with frequent desire to void urine, followed by increased suffering when he had emptied his bladder. He also felt a call to discharge his bowels whenever micturition was demanded. He remained more or less in this condition until about two months prior to admission into hospital, when his sufferings became greatly aggravated. He was then

pressed every half hour to make water, which was always bloody, and with a slightly fetid odour. He could not pass it with any ease unless when lying almost on his face, or partially on his left side; and his sensation was as if his bladder was not quite emptied, followed by painful and continued straining, together with the flow of some drops of pure blood. He described his agony as intense when he sat on the shaft of his car driving. He never had retention of urine, nor did he ever notice that the stream of water stopped suddenly in passing. His health, however, was gradually failing, and he became unable to perform any work. A week before he came to the hospital he could not retain his water, which dribbled away at night, and he was also annoyed by painful erections. When he came under my care all the foregoing symptoms were prominently developed; and on examination it was found that he had a large bunch of external hemorrhoids, and that the glans penis, which was quite uncovered by prepuce, was looking moist and excoriated, and the lips and orifice of the urethra red and slightly everted. On the 21st September, with the assistance of my colleagues, I sounded him, being unable to pass a sound larger than No. 9, in consequence of the small size of his urethra, which was throughout narrow, though no abrupt stricture existed. The instrument on its introduction caused him a great amount of pain, but it immediately struck the calculus, giving proof of its presence. I withdrew the sound, and endeavoured to measure the stone with Mr. L'Estrange's ingeniously-constructed lithometre, but I was disappointed, as I could not grasp it. The sensation, however, which was given from passing the instrument over the surface of the stone led to the impression that it was of considerable dimensions. His urine was acid, and contained some pus globules. It also became slightly clouded when heat was applied, but only to as great an extent as could be explained by the presence of the purulent matter. On the 1st October, having his rectum cleared by an enema, and himself placed under the influence of chloroform, I proceeded to cut him for the stone. The man having been tied in the usual manner, I injected three ounces of tepid water into his bladder, and a No. 9 staff, grooved on the side, having been introduced, and admirably held by the late Mr. Smyly, the patient's buttocks were brought a little over the edge of the table. Kneeling then on my right knee, I commenced an incision with Mr. Fergusson's knife one inch and three quarters in front of the anus, to the left of the raphe; this I carried down midway between the tuberosity

of the ischium and the anus, fairly passing both. By so doing it will be evident that the length of the cut was considerable; and I deem it of great importance to make the first incision a free one; (nearly three inches in the adult). I then divided the deeper structures with one or two light strokes of the knife; and having felt for the groove of the staff with the forefinger of my left hand, the nail of which I firmly fixed in it, lowering my wrist I thrust the lateralized knife from the *lowest part* of the wound upwards, and lodged its point in the groove, transfixing the urethra at its membranous portion. I then steadily pushed on the knife along the groove into the bladder, dividing partially the triangular fascia, levator ani, prostate, and neck of bladder. A small quantity of water now escaped along the knife, when I rapidly withdrew the instrument, slightly making it cut its way out. I now laid aside Fergusson's knife, and passed Crampton's lithotome along the groove of the staff into the bladder, and carefully sent the index finger of my left hand along its back, and thus cut the prostate to a sufficient extent. Having withdrawn the lithotome, the cutting part of the operation was concluded. Keeping the index finger of my left hand in the wound and bladder I turned it twice round to dilate the parts, and then, having felt the stone, desired the staff to be removed. On the finger so placed I introduced a blunt gorget, along which, when I withdrew my finger, a large gush of water flowed. I then, guided by the gorget, introduced the forceps, took out the former, and keeping the forceps closed until I felt the stone, opened its blades, seized, and extracted the calculus. I then sought to ascertain if a second stone was present, but not finding any, I examined the wound, and there was no bleeding. I at once introduced, and fastened in, a tube, placed a suppository containing a quarter of a grain of morphine in his rectum, and caused the patient to be untied and removed to bed. It would be tedious to note from day to day the changes in this case. His progress to perfect recovery was rapid, and no bad symptom occurred throughout. The stone weighed in air 688 grains, in water 353 grains. Specific gravity 1·938. A tolerably faithful drawing of it is given in Plate I., Fig. 1. The man left the hospital, perfectly cured, on the 27th of November.

Case II.—*A Boy aged Six years; Lateral Operation; Recovery.*—Patrick G. was admitted into the Meath Hospital, May 20th, 1862, suffering from symptoms of stone in the bladder, such as very

frequent calls to pass water for the preceding twelvemonths, great pain after the bladder was evacuated, and at times, prolapsus ani when straining to void urine. He was constantly pulling at his prepuce, which was much elongated, and slightly excoriated. He complained occasionally of severe pain in the lower part of the abdomen, and used to expel large quantities of flatus. His bowels were at times very loose, and he often picked his nose until it bled. There never was any blood noticed in his urine, nor did the stream of water ever stop suddenly, but at times it passed in a quivering interrupted stream. On the 21st May I had this boy chloroformed, and sounded him; but did not detect the presence of the stone. I had his pelvis then raised at least four inches above the level of his shoulders when the sound struck the foreign body, and declared its presence in the bladder.

Operation.—May 30th. His rectum having been cleared out by an enema of thin gruel in the morning, he was chloroformed, and two ounces of tepid water were thrown into the bladder by means of a catheter and syringe. A staff, laterally grooved, of the largest size his urethra would allow was introduced, and intrusted to the care of Mr. Wharton, who held it up well to the arch of the pubis, and he was tied in the usual manner During a spasm some of the water escaped, and prolapsus ani occurred, but the bowel was with facility returned, and by gentle pressure remained in its place. I commenced an incision with Fergusson's knife, at the left of the raphe, about three-quarters of an inch in front of the anus, and carried it backwards to the extent of two inches. I then gave the deeper structures one stroke of the knife; and having felt for the groove of the staff, I fixed the nail of the left hand fore-finger in it, and immediately struck upwards with the point of the knife lateralized for the staff. Having placed the knife safely in the groove, I moved it from side to side, making sure that it was in its proper position. I then pushed it on to the bladder, dividing the neck of it, and the little prostate; withdrawing the knife, I gently inserted the fore-finger of my left hand, felt the stone, and directed the staff to be removed. On my finger I introduced a small gorget, and along it a forceps, grasped the calculus, and delivered it. I then felt for a second stone, but none being present, I examined for bleeding, and found some slight oozing, but no hemorrhage from any particular point. The *canule à chemise*, well oiled, was then inserted. The patient was at once placed in bed, with a waterproof sheet, and a folded blanket under his hips.

May 31st.—He had passed a tranquil night, and the water flowed freely by the canula. Daily the little fellow improved in health; the urine passed freely through the penis on the sixth day after the operation, and he left the hospital cured, August 6th.

My friend Professor Haughton kindly examined the calculus, a drawing of which I have added, Plate I, Fig. 3, when the following was the result:—

Weight in air, 328·3 grains.
Weight in water, 130·1 grains.
Specific gravity = 1·656.

The central nucleus was $\frac{1}{4}$th the linear diameter of the calculus, = to $\frac{1}{64}$th of its entire bulk, and composed altogether of oxalate of lime.

The exterior portion of the stone was concentrically arranged, porous, composed of lithic acid, with small portions of lithate of ammonia, phosphates of lime, and magnesia

Case III.—*Man aged Twenty-five Years; Lateral Operation; Recovery.*—John M'N., a baker, was sent to me from the county Tyrone, and admitted into the Meath Hospital, September 12th, 1864. The following were the symptoms under which he laboured:—Frequent inclination to make water, and intolerable pain when the last drops were being expelled; a dull pain in the lower part of the abdomen; occasional attacks of tenesmus. He was very much emaciated, and his countenance indicated suffering. He had symptoms of irritation of the bladder for two years previously, but never had retention of urine, or sudden stoppage of the stream. I sounded him, and with ease detected the calculus. The examination gave a good deal of pain, and some blood followed. He was ordered a hip-bath, and a mixture containing camphor and hyoscyamus, and the next day I found him easy.

Operation.—September 27th.—At 7 o'clock, a.m., he got an enema, which washed out his rectum thoroughly, his perineum was shaved, and at ten o'clock I had him brought into the operating theatre. I then passed a catheter, and drew off urine, and immediately afterwards injected about five ounces of tepid water. He was then put under chloroform, and whilst this was being accomplished, he was tied, and a good-sized staff, grooved on the side, introduced. With the staff he was sounded, the stone was felt, and the instrument was intrusted to my colleague, Mr. Wharton. His buttocks having been placed a little over the edge of the table,

and held perfectly straight before me, I commenced an incision with Fergusson's knife, to the left of the raphe, about one inch and three-quarters anterior to the anus, and carried it backwards half way between the tuberosity of the ischium and the anus. I then went deeper, by two or three touches of the knife, over the same track. I felt with the left fore-finger for the staff, and having fixed my nail in it, lowering my hand, and lateralizing the blade of the knife, I struck upwards, and lodged the point in the groove, and pushed the knife along it into the bladder, until I felt it arrested by the stop at the end of the staff. A little water flowed at this stage of the operation. Laying aside the knife, I passed in the lithotome, and my left fore-finger was inserted after it along its back, and divided the prostate; I then withdrew it, still keeping my finger in the bladder, with which I could distinctly feel the stone. I then requested the withdrawal of the staff, and along my finger passed in a gorget. A copious flow of water now coursed along the instrument. I introduced the forceps, laid down the gorget, seized a stone, and extracted it. A facet was observed on the calculus; and on introducing my finger, a second one was detected. The forceps was again introduced on my finger, and the other stone delivered. I now examined for bleeding, and, as there was none of consequence, I introduced a tube, tied it into the bladder, placed a morphine suppository in the rectum, had him untied, and conveyed to bed.

Sep. 28th.—He had passed a good night, the urine coming freely through the tube and wound.

Sep. 29th.—Felt a little feverish to-day; there was no pain; the stomach inclined to be sick. Ordered him small effervescing draughts, containing tincture of opium, and ice.

Sep. 30th.—Much better this day; he had a good night, free from all pain. I then removed the tube. This man progressed most favourably to recovery. The urine passed through the penis on the seventh day after the cutting, and he was discharged cured on the 5th December.

Figures 4, 5, and 6, Plate I., give very accurate representations of these stones. Fig. 4 exhibits the facet, Fig. 5 the manner in which they lay together in the bladder, and Fig. 6 a section which shows a cavity in the centre, proving that the nucleus was organic, and not from the kidney. I am indebted to Professor Haughton for the following qualitative analysis:—

Composition — Principally phosphate of lime, with traces of

phosphate of magnesia and lithates. They are rather rare urinary calculi, as they consisted so much of phosphate of lime.

Case IV.—*A Youth aged Sixteen Years; Lateral Operation; Recovery.*—Christopher C., a native of the County Wicklow, was sent to be placed under my care, and admitted into the Meath Hospital November 3rd, 1864. On his admission, all the rational symptoms of stone in the bladder were present. Assisted by my colleagues, I sounded him the day after he came in, and instantly detected the presence of a calculus. He had suffered for two years from vesical irritation, but the last two months before he came into hospital the pain he felt after micturition was most poignant. I had him placed in a warm bath at night on four different occasions, and gave him medicine likely to soothe the irritable condition of his bladder. On the 16th of November I proceeded to perform the operation of lithotomy for his relief. His rectum having been thoroughly evacuated by a simple enema, I had him brought under the influence of chloroform, and tied on the table of the operation theatre. I injected about four ounces of water into his bladder, having previously drawn off all urine. Having done this, I at once introduced a large-sized staff, grooved on its side, into the bladder, and with it sounded for, and felt the stone. And this is a preliminary never to be forgotten by the surgeon. It is no rule of my laying down, for every good lithotomist has enforced it. How fearful would be the mistake of cutting into a bladder and finding it free from the presence of the stone which, the very day before, had been distinctly felt! The possibility of passing one of small dimensions must never be forgotten. Convinced, in this case, of the existence of the calculus, I knelt on my right knee, laid Fergusson's lithotomy knife on his perineum, a little to the left of the raphe, and carried it back well beyond the anus, midway between it and the tuberosity of the ischium. Having deepened this incision sufficiently to fix the nail of my left fore-finger in the groove of the staff, I thrust the knife, lateralized, from the lowest part of the wound upwards and backwards, and transfixed the membranous portion of the urethra in the manner already described in the three former operations, and concluded the extraction of the stone in the same way. Assured by a careful examination that no second calculus was present, I looked for bleeding; and as none was visible, I put a tube in the wound, tied it in its position, and had the boy carried to his bed, which was prepared with a waterproof

sheet, and folded blanket beneath his buttocks. The only peculiar feature in the operation worth noticing was the fact that I caught the stone in the long diameter, and was in consequence obliged to use a good deal of force with an up and down motion of the handles of the forceps before it was completely delivered. This lad recovered most favourably. From the day of the operation he steadily improved, without the supervention of any unfavourable symptom, and left the institution, cured, on the 2nd January, 1865. A very faithful coloured lithograph of this calculus is given in Plate I., Fig. 2.

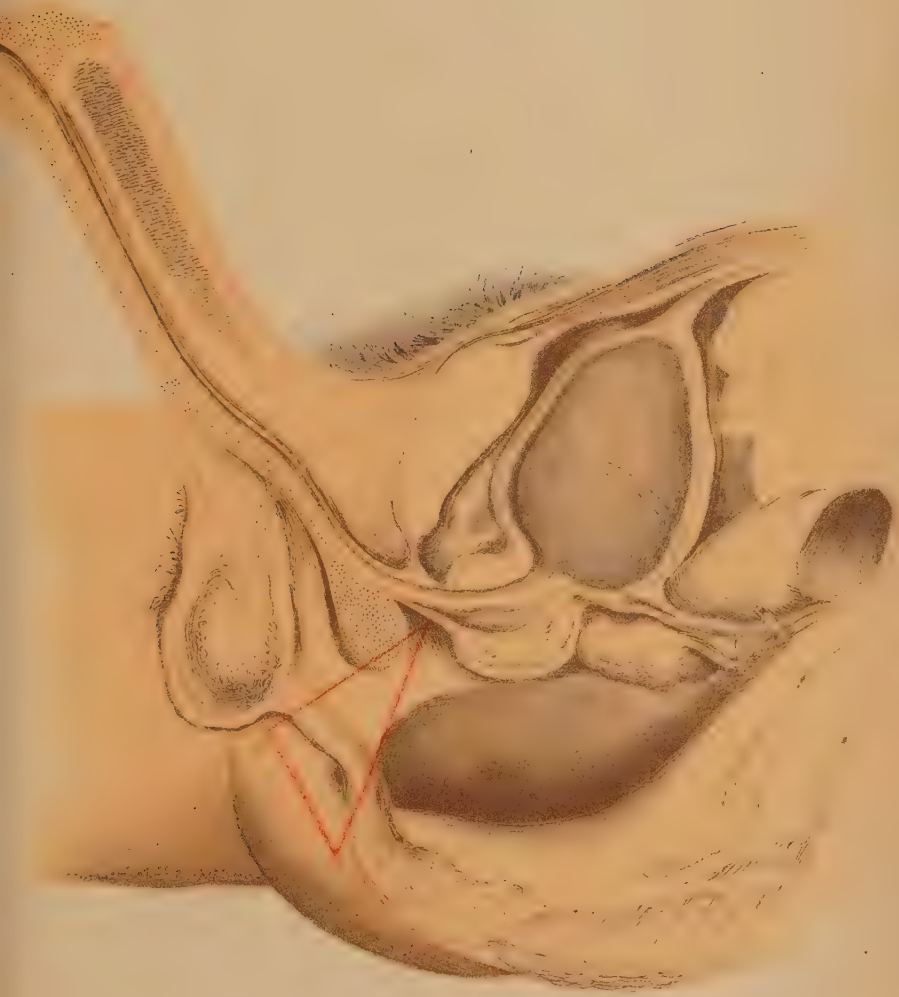
The practical points that I would urge on those who may be called on to perform the operation of lithotomy, are mainly with reference to the form of the staff, the injection of the bladder, and the manner of opening the urethra prior to cutting the prostate gland.

Differently-shaped staffs or guides into the bladder have been recommended to the surgeon who cuts for stone. Amongst those most lauded I may mention the ordinary one grooved on its convexity, the almost straight staff of Key, the rectangular of Buchanan, and Liston's, grooved on the side. The last named I have always used myself, and have no hesitation in giving my opinion in its favour, as possessing superior advantages. I believe the side-grooved staff is more easily struck, or reached, than those which are grooved on the convex part. But a still more important point in this construction is, that it more or less enforces the lateralization of the knife on its way into the bladder, thus keeping off its edge from the rectum. I have never seen this bowel wounded when Liston's staff was employed.

With regard to injecting the bladder before the operation, I must lightly approach this question. Some surgeons believe that an empty condition of the viscus is to be preferred, because it allows the stone to lie close to the neck of the bladder. On the other hand, it has been recommended to permit urine to collect for an hour or two previous to cutting. I must say that I consider it is far better to cut upon a bladder containing fluid, than when it is in an empty and collapsed state. It is more in its proper position when moderately distended; it is also more steady and resisting to be cut into, and the gush of fluid unmistakably proclaims that the operator has driven his knife in the right course. But it is better to evacuate all urine, and to throw tepid water into its place. A great deal has been written concerning the danger of infiltration of urine

as a consequence of lithotomy, and much stress has been laid on the extent to which the prostate gland should be divided in order to obviate this unhappy occurrence. I feel convinced that too much credence has been given to this notion concerning the division of the gland beyond the limits of its capsule. Of course this division should be avoided, and it is not necessary unless the stone be of enormous size. My impression is (but I by no means put it forward as a positive doctrine) that infiltration is often produced by a want of correspondence in the different incisions as the knife travels to the bladder, and that it occurs almost immediately with the first escape of urine. Every surgeon who has seen cases of extravasation of urine from ruptured or lacerated urethra, knows its baneful effects on the parts it touches, and how quickly it begins its action, ending in death of those parts. Now it strikes me that should the incisions not correspond, or be carried beyond the proper extent in the prostate, the contact of tepid water is safer than urine; and that the introduction of a tube into the incision and bladder, before the urine can be collected in any quantity, may possibly avert the dangerous consequences of a sudden gush of this secretion into the parts around the wound.

The last point on which I would dwell is the manner in which the operator should direct his knife when striking for the staff. The part of the urethra that should be opened is the membranous; and if for a moment its position be considered, overlapped more or less by the bulb, it will be seen that it is by no means an easy procedure to transfix it without wounding the bulb, and its artery. No doubt it has often been laid down by excellent surgeons that it is almost impossible to avoid wounding the artery of the bulb, and that such injury is of little moment. I maintain, nevertheless, that, if possible, such should not be done. To the very young or old patient the loss of blood is always a source of danger; and even regarding bleeding from the artery of the bulb in the lightest manner, it must be allowed that it is a most disagreeable and unhappy occurrence. I conceive that the operator may avoid this if, when striking for the staff, he directs his knife, properly lateralized, upwards and backwards from the *lowest* part of the first incision. Plate II. will explain my meaning. It will be seen that the wound in lithotomy is triangular in shape, the apex, B, at the membranous part of the urethra, and the base formed by the external incision, A C. It is quite evident, looking at this, that a line of incision passing to the membranous part of the urethra, from any



part of the wound except the lowest, must strike the bulb, but that if the knife be directed up from the lowest part it passes behind this overlapping portion. In order to accomplish this part of the operation it is necessary to lower the wrist when making for the staff, and therefore I consider the kneeling posture the one which brings the operator well under the bladder, and enables him most easily to perform this important step of his operation. I have no doubt but that this procedure also materially aids in the prevention of infiltration of urine, by making the wound into the bladder more direct and depending.

2.—Aneurism of the Popliteal Artery cured by Digital Pressure in Twenty-six Hours and Forty-five Minutes.

The cure of aneurism by any form of compression will always be regarded with considerable interest by the Irish surgeon, as among the many improvements in modern surgery, and as one which we are entitled to claim as essentially our own. The simple, and almost painless, pressure of the human finger, producing a rapid and easy cure of a popliteal aneurism, is well exemplified, I conceive, in the following case, which I had under my care in the Meath Hospital.

George K., aged sixty-six years, was admitted to the hospital April 18th, 1865. The history of his case was briefly as follows:—Five years of his life, up to ten months prior to seeking advice, he had been employed driving a car. The seat from which he drove he described as being too low for a man of his height, and causing his knees to be bent and cramped considerably. His employer's death, however, compelled him to give up this occupation and adopt that of a labourer, when he had occasionally to walk great distances. Towards the end of December, 1864, he suffered greatly from a cramp which extended down the back of his right leg, from the knee to the heel. He did not, however, at that time seek any remedy, but endured the pain until the middle of February, 1865, when he noticed a pulsating tumour in the right ham. He was annoyed by feeling a throbbing in the part, and stiffness, particularly after sitting a short time. On the day mentioned he sought relief for a hydrocele of the tunica vaginalis testis, and complained of a swelling, and throbbing behind his right knee. The main symptom present on admission was a pulsating tumour, the size of a small hen egg, in the right popliteal space, and the circumference

of the right limb (measuring over the centre of the tumour), was one inch larger than the left. The tumour became smaller and softer when pressure was made upon the femoral artery at the groin, and at the same time all pulsation ceased. The pulsation was eccentric and synchronous with that of the heart, and a *bruit de soufflet* was audible on applying a stethoscope to the tumour. There was not the least amount of œdema of the right leg or foot observable. The patient was a good-tempered old man, and when the nature of his malady was explained, and the mode of treatment mentioned, he at once promised to aid in every way by obedience to my orders. On a careful examination of his chest and abdomen no other aneurismal development could be discovered. Considering this a very suitable case to try digital pressure, I commenced it on the morning of the 19th April, at 11 o'clock, a.m., a number of the pupils of the hospital kindly volunteering their assistance, and each gentleman becoming responsible for two hours pressure at a time, up to 9 o'clock, p.m. Having applied a flannel roller from the foot to the knee, and shown the pupils how to make pressure by placing the points of the middle and index fingers of the left hand over the femoral artery where it crosses the brim of the pelvis, then to fix and increase the pressure by placing the middle and ring fingers of the right hand on those already over the vessel, I desired that the pulsation in the tumour should be arrested as much as possible. I also taught the patient himself to press the vessel, and throughout the treatment of the case he occasionally gave assistance to the young gentlemen in charge. I visited him at 2 o'clock, p.m., April the 19th, and learned that the compression had been carefully sustained, and was well borne by the patient. The tumour was now certainly more solid than before, but the pulsation remained undiminished whilst the leg and foot affected were warmer than the other. Again, at 5 o'clock, p.m., of the same day I saw him. The solidity of the aneurism had by this time evidently increased; but as yet no apparent change had taken place in the force of pulsation. 9 o'clock, p.m. The tumour now was much more solid. The pressure was then discontinued for the night, and thirty drops of Battley's sedative liquor of opium administered in a draught.

April 20th, 9·30 o'clock, a.m.—The man had passed a tolerably good night; awoke once about midnight with a sharp pain in the tumour. This was the first time he had ever felt pain in the aneurism, but it did not last more than half an hour, when he fell asleep. Compression was resumed this day, by gentlemen who

came forward to offer their assistance, as others had done on the day before. 12 o'clock, p.m.—The tumour now was very solid, and the pulsation was greatly diminished. 1·45 o'clock, p.m.—Pulsation had now ceased completely, and the sac was firm, and solid. The patient complained of slight numbness along the back, and outside of his leg, but there was no œdema whatever in leg or foot. The pressure was continued in a slight degree up to 7 o'clock that evening, when it was finally discontinued. The beating of the femoral artery could be traced down at this period to about four inches from the knee. He got the opiate, as before, at 9 o'clock, p.m.

April 21st.—He had passed an excellent night; no pain or swelling in the limb. The superior internal articular artery was evidently enlarged, and pulsating strongly. I kept him in bed for a fortnight from this date, with the limb rolled in flannel, and the tumour gently rubbed, every night, with a liniment, consisting of 2 oz. of soap liniment and 15 grains of iodide of potassium. He was then allowed to get up, and move about the ward on crutches.

May 29th.—I tapped his hydrocele; and on the 3rd of June he was discharged the Hospital.

Thus, in the short time mentioned, this formidable disease was perfectly cured by finger pressure. The point where the fingers rested was only slightly reddened, but did not become sore throughout the treatment; and he never complained that the pressure gave him pain. This contrasts strongly with instrumental arrest of the flow of blood through an aneurism; for no matter how carefully applied, it cannot be borne so long as can the finger top; and further, the artery cannot be isolated so completely from its accompanying vein by mechanical as by digital compression, and therefore, œdema of the limb is more likely to ensue. There is less likelihood of ulceration, or sloughing of the skin occurring under the point of pressure than when an instrument is used, inasmuch as by being so direct, a smaller amount of force suffices to control the current of blood through the artery; and, as the point of pressure can with such ease be changed to a higher or lower position. But in addition to all this, the rapidity of the cure in cases of digital treatment contrasts most favourably with any mechanical pressure, as in one case a few hours usually accomplish what in the latter requires days. With respect to the rapidity of cure in cases of aneurism treated by compression of any kind, I cannot more appropriately conclude this account of mine, than by citing some remarks of

my esteemed friend and very accomplished surgeon, Mr. L'Estrange, at a recent meeting of the Surgical Society of Ireland. They struck me forcibly at the time, and are well worth the attentive consideration of every practical surgeon. I quote from the *Dublin Medical Press*, June 7th, 1865, p. 536. He said:—"I would wish to observe that it appears to me that there must be some peculiar state of the blood, which admits of quicker coagulation in some individuals than in others, placed under similar circumstances and under similar treatment. For instance, we have in this city, within these last few months, three cases of aneurism treated by external compression. In one, cure by coagulation of the contents of the sac was effected in the short space of five hours and three-quarters (Dr. Mapother); the second was effected in twenty-five hours and a-half (Mr. Porter)—(here he slightly erred); and in the third, coagulation could not be effected although the external compression was steadily kept up, and well borne by the patient for many weeks. It would be of great importance to ascertain this physiological and pathological fact, as to the coagulation of the blood in the aneurismal sac under compression. This, I should think might be ascertained, or great light thrown on the subject, by taking a small quantity of blood from each of these patients—Dr. Mapother's, Mr. Porter's, and the one in the City of Dublin Hospital—in which coagulation would not take place by external compression; and by submitting their blood to chemical and microscopical tests, much information might be obtained, which would conduce materially to the scientific treatment of aneurism."

3.—CASTRATION; THE VESSELS OF THE CORD SECURED BY ACUPRESSURE.

This new mode of arresting hemorrhage has not yet been much adopted in this country, although emanating from the brilliant genius of Professor Simpson, whose name must give weight to any medical or surgical suggestion. In Dublin, the first surgeon who used acupressure, according to this modern plan of Simpson, was my esteemed colleague, Professor Macnamara, in a case of amputation of the breast, and with perfect success. He has also by this means arrested the bleeding from a stump, after amputation through the knee. I have also seen this method in the hands of my friend Mr. Banon, successfully restrain bleeding from a vessel deep under

the tongue, when he removed a large portion of the front of the lower maxilla. In the following case I tried it myself with happy results.

Thomas T., aged forty, a drummer in a militia regiment, was admitted into the Meath Hospital Feb. 20th, 1865. He stated that eight months previously he had received an injury of the left testis, by knocking it against the corner of a large chest. On the next day acute orchitis set in; the testicle soon suppurated; shortly afterwards the wall of the abscess became attached to the scrotum, which at last gave way, when a large fungoid growth protruded. On admission the tumour seemed to be that termed lipoma, and from its size the whole testicle appeared unravelled, and deranged. Its surface was irregular, of a dirty ash colour, and it exuded a thin fetid discharge. It gave the patient little or no pain; his health was not impaired, and there was no glandular enlargement in the groin or abdomen. Various astringent applications were used to restrain its growth, prior to the performance of Syme's operation of paring the edges of the scrotal aperture, pushing back the testicle, and stitching the edges of the scrotum over it. These appliances had no beneficial effect; and as he wished to be with his corps while under training, he insisted upon the more speedy cure by extirpation. Accordingly, on the 7th April, he being under the influence of chloroform, I performed the operation in the usual manner, by an incision commenced at the external abdominal ring, carried to the tumour, and then from the lower part of the growth, about half an inch towards the bottom of the scrotum; a longer incision not being required as the testicle protruded. Having then separated the testis from its bed, I isolated the spermatic cord, and passed under it a needle with a wire attached. Having then passed a wire loop over the point of the needle so placed, I brought it in front of the cord, and winding it under the other end of the needle (after Simpson's third plan), I tightly secured the entire cord between the needle and wire. I then divided the cord below the needle, and found the vessels perfectly restrained, without exuding a drop of blood. In addition to the arrest of bleeding, the needle, which lay across the top of the wound, prevented any retraction of the cord. The end of the cord was kept in view in such a way that if any bleeding had taken place the vessels could have been tied with the greatest ease. There was now only one other artery at the septum scroti to be secured. It was arrested by acupressure in the same way.

Having then shortened the needle with a nippers, I brought the wires out through the wound, and approximated the edges by three points of silver wire suture. April 9th.—I removed the needles with the greatest ease; the patient felt no pain, nor did a drop of blood follow. The wound healed kindly, a moderate amount of suppuration taking place. It was perfectly cicatrised on the 29th April, and the man was discharged from the hospital on the 5th May. From my limited experience of this method I could not venture to recommend the surgeon on all occasions to adopt this mode of sealing vessels, though nothing could be more satisfactory than its action in this case just related. One of the objections to acupressure is the pain produced by pressing on nerves; yet here the entire spermatic cord was enclosed between the needle and wire without causing much annoyance. The patient never complained of pain after he recovered from the anesthetic sleep. From my own experience, and that of some other surgeons, the great objection that might be urged to acupressure is that it is certainly more *tedious* than the ligature. But I have no doubt that we sometimes find bleeding vessels in situations where it will be found far easier to control the hemorrhage by this means than the ligature.

ART. II.—*The Climate of Malaga, and its Influence on Chronic Pulmonary Diseases, especially Consumption.* By THOS. MORE MADDEN, M.D., M.R.I.A.; L.K. & Q.C.P.I.; M.R.C.S.E.; L.F.P. & S., Glasg.; Demonstrator of Anatomy in the Carmichael School of Medicine; author of *Change of Climate: a Guide for Travellers in Pursuit of Health*, &c.

THE following communication on the medical climatology of the city of Malaga, and its influence on health and disease, is founded on my experience of this climate during a considerable part of three winters; and in part, also, on long residence in various parts of the south of Europe and Northern Africa, which afforded opportunities of comparing the climate of Malaga with that of other localities resorted to in winter by British pulmonary invalids.

Since last year, when I published an account of this health resort in my work "On Change of Climate," I have been favoured by the observations of the most recent meteorologists in Malaga,

the results of which are embodied, with my own hitherto unpublished notes, in the present paper.

Before speaking of the climate of Malaga I shall, however, make a few remarks on some of the difficulties that embarrass the study of medical climatology, and some fallacies which I conceive prevail in this science, and then very briefly point out the climates which my experience leads me to believe are best adapted to the various forms of pulmonary disease.

The majority of writers on medical climatology are physicians established in practice in the southern *sanatoria* resorted to by invalids in pursuit of health, who, having themselves derived sanatory benefit from the climate in which they have settled, are, very naturally, disposed to regard that place from the most favourable point of view, and often, unconsciously, exaggerate its good qualities or fail to point out its defects. Moreover, many of these gentlemen do not appear familiar with any southern winter residence except that which they describe; and, although some of the local works on climate contain very lengthened, reliable, and valuable observations on the state of the weather and atmosphere, yet such observations by no means compensate for the want of those properly instituted meteorological comparisons which it requires an extensive personal knowledge of many climes to carry out effectually.

Mere tables seldom convey any accurate idea of the climate of a place; for, as a very able writer on the "English Exodus," in *The Medical Times and Gazette*, Oct. 15th, 1864, justly observes:— "Our sensations of heat and cold do not always tally with the rise and fall of the thermometer, nor our impressions of the lightness or heaviness of the atmosphere with the variations of the barometer; the sirocco may produce an intense feeling of lassitude and dejection, and yet the barometer hardly vary a line."

The point on which invalids going abroad really require information is the temperature of the winter and spring months in the place where they intend sojourning; but in works intended for such persons this point is generally passed over with a reference to some table of temperature, which conveys no distinct idea to the mind of the ordinary reader; therefore, in the following remarks I have endeavoured to compare the temperature, state of the weather, prevailing winds, &c., in Malaga with other known winter climates.

Fashion has a great influence on the reputation of the climates

frequented by English valetudinarians; thus Cannes owes its *renommée* as a winter residence mainly to the influence of a celebrated nobleman, who, some thirty years ago, passed a winter there, and, finding the climate suited him, has since returned each year, followed by others—so that the climate that suits a very distinguished person seems to possess some special attraction for English invalids. A medical man may, perhaps, attend in the wake, to look after the bodily infirmities of his exiled compatriots; and the doctor, having probably some time on his hands, writes a book in which he proves the superiority of the climate to his own entire satisfaction; and in some cases a very bad climate for phthisical invalids—as, for instance, Mentone—has thus been "written up" into temporary notoriety. Unfortunately laudations of unsuitable climates are seldom contradicted. Valetudinarians, having little the matter with them, visit Mentone and similar places, and, being benefited by travelling, and change of scene as well as of air, the result is vaunted as an incontestible proof of the sanative action of the climate; but we hear nothing of the cases in which invalids suffering from organic disease are injured by an unsuitable climate, and die in these localities. Thus it is that physicians at home are misled, and their patients abroad suffer the consequence.

Too much importance is attached to the mere temperature of a place in considering its eligibility as a residence for invalids. The influence of a climate on health and disease is the impression produced on the body by the state of caloricity, humidity, and electricity of the atmosphere—its pressure, purity, and amount of ozone it contains, all of which are modified by the topographical conditions of the locality. Now, the thermometer, so generally relied on as the test of climate, only indicates one of these conditions. It is obvious, however, that persons suffering from phthisis, chronic bronchitis, asthma, some periodic nervous affections, and, in short, almost all chronic diseases in which change of climate is resorted to, are highly sensible of, and are acted upon by all changes in the electrical and hygrometrical state of the atmosphere, its purity, the direction and force of the winds, and many other circumstances, none of which are in any way indicated by the thermometer.

When we have to choose between the various southern climates usually resorted to by British invalids in any case of pulmonary disease, the division of such climates into two great classes—*i.e.*, *tonic* or dry, and *sedative* or humid—becomes of most important practical assistance to the often embarrassed practitioner. And yet,

though four years have now elapsed since Dr. Corrigan urged the importance of this classification, in his admirable address to the Irish College of Physicians, to the present day it is not by any means sufficiently recognized in practice—people resting satisfied with removal to a climate so many degrees warmer than that of these islands, without taking into consideration the other equally important requisites of a good winter residence. In a valuable *brochure*, reprinted from *The Medical Mirror*, for May and June, 1864, "On Change of Air," Dr. Thorowgood has also ably demonstrated the great utility of Dr. Corrigan's simple division of southern climates.

The year before last a case fell under my notice which forcibly impressed me with the justice of Dr. Corrigan's observations. In this instance the patient was a gentleman, aged thirty-seven, of a scrofulous diathesis, and phlegmatic temperament, suffering from the first stage of phthisis, marked by symptoms of great weakness and relaxation. In Malaga, where I had attended him during the preceding winter, he had derived so much benefit from that tonic, warm atmosphere that I thought it very probable that, by observing proper precautions, he might yet enjoy many years of comparatively healthy existence. Such was Mr. R.'s condition when I saw him in May. Five months subsequently, in December, I received a letter from him telling me that he was dying, and that ever since his arrival in Alexandria, where he had been sent by his medical adviser in England, his cough and dyspnea had become urgent, the expectoration had increased and become purulent, night sweats had set in, which, alternating with diarrhea, were rapidly exhausting his strength. I wrote at once, urging him to move up to Cairo; but it was too late, and shortly afterwards I heard of his death. Now, if a dry climate had been selected in this case, instead of an extremely humid one, the result would probably have been different; for, although Upper Egypt enjoys, perhaps, the dryest atmosphere in the world, the air of the delta of the Nile, especially at Alexandria, is, particularly during certain winds, laden with vesicular moisture.[a]

[a] I find the following corroboration of this opinion in an ably written notice of the cholera now raging in Lower Egypt, published in the *Morning Star* London newspaper of the 1st of July, 1865, wherein a remarkable observation of a writer on Egypt, whose work was published thirty-four years ago, is thus cited:—"As an example of the exceeding humidity of the climate, we may quote a passage from Dr. R. R. Madden's work on Egypt. 'In Alexandria, when the air is excessively moist, I observed severa mummies melt away in a dark magazine where I kept them, and decomposition took place after an exposure of forty hours to the humid atmosphere, though the same bodies had resisted corruption in a dry air for perhaps forty centuries.' This fact is a most

The hygrometric state of the air, however, is not the only point on which errors are committed in the selection of climates for pulmonary invalids. When I was in Pau patients complained to me that hemoptysis had attacked them more severely, and at shorter intervals, after their arrival there than before they had left England. This result might have been anticipated had the practitioner who recommended that climate to a patient subject to pulmonary hemorrhage been acquainted with the effects which the thin, rarified atmosphere of a mountain district must produce on the capillaries of the lungs. I mention these instances only as a proof that climatology should form as essential a portion of medical study as any other branch of therapeutics.

Change of climate is resorted to in two varieties of phthisis, and is productive of equal service in both. As, however, the climate that would be most advantageous in one form of the malady would probably only hasten death in the other, I shall briefly point out the characteristics of both these forms of tubercular disease of the lungs, and the climates that are best suited for their cure or alleviation. A very considerable portion of the phthisical patients that are sent abroad from this country require a dry, warm, bracing, or tonic and equable atmosphere, being persons in a relaxed anemic condition, often of scrofulous diathesis, who suffer from profuse expectoration, and are weakened by night sweats or diarrhea. The principal *tonic* winter residences for these patients are Western Australia, Upper and Middle Egypt, Nice, Hyères, Cannes, Montpellier (during a part of the winter only), and Malaga. The second class of cases which are met with in the health resorts of Southern Europe and Africa are marked by frequent hard dry irritative cough, scanty expectoration, pulmonary hemorrhage, and fast irregular thready pulse. In such cases any of the climates I

remarkable one, and illustrates effectively the nature of this Alexandrian atmosphere. It is equalled by Dr. Barclay's assertion that during the time he was there he felt as if inhaling steam; his breathing was excessively affected, and his whole system was languid and relaxed. If we have thus endeavoured, at some length, to lay before the public the real hygienic condition of Alexandria, it is because we wish it to be understood that the presence of even a violent epidemic in that city is nothing extraordinary. IF REPORT RE TRUE, one hundred and eighty-four persons died of cholera on Sunday last in the hospital of Alexandria. The entire number of deaths recorded up to the present is between a thousand and fifteen hundred."

I may, however, observe that the writer of the article in the *Star*, to which I have referred, has forgotten to acknowledge the source of his information about the climates of Egypt, which he has done me the honour of taking, almost *verbatim*, from my work.

have just mentioned would prove too exciting. The locality selected for the winter residence of these patients should have a *sedative*, warm, and somewhat humid atmosphere; and the physician may choose between Madeira, Pisa, Rome, and in some instances Lisbon, according to the symptoms of each case. Algiers holds a kind of intermediate place between tonic and sedative climates, in some respects approximating to Upper Egypt and Malaga, while differing from them in others—the atmosphere being in many cases relaxing. It is occasionally very serviceable in both varieties of phthisis pulmonalis.

The division, generally adopted, of phthisis into three stages is of practical utility in the application of change of climate to the cure of the disease, each advance in the progress of the complaint modifying the selection of a suitable climate. I cannot, however, enter on this question here, but shall only say that, to be used with success, change of climate must be early had recourse to, and that nothing can be more cruel than to send a patient, advanced in the last stage of consumption, abroad, as a last resource, when the case has really become hopeless. I have, unfortunately, too often had to witness the deathbed of a consumptive patient who had been sent abroad when this measure, instead of being useful, must have been hurtful, and when death was probably hastened by the fatigue and excitement of the journey.

Facility of access is a point of great importance in the selection of a climate for invalids. It is obvious that a patient in an advanced stage of disease should be sent to a place which may be reached by an easy and agreeable journey, and that in case of need he may communicate quickly with his physician and friends at home. In this particular Malaga will bear comparison with any health resort on the Mediterranean, as it may be readily reached by either land or sea. Having more than once tried each route, I have no doubt of the advantages of the sea passage for invalids. The overland journey *viâ* Madrid and Granada should never be attempted by those in delicate health; the quickest route for such individuals is that *viâ* Paris and Marseilles by rail, and thence by steamer, touching at Barcelona, Valencia, and Alicante, to Malaga. There is also constant steam communication with the south of Spain from England, either by the Peninsular and Oriental Steamers to Gibraltar—only 80 miles to the east of Malaga—or directly to Malaga from Liverpool, and also from London. I should in general recommend the Liverpool route to consumptive patients. This

passage occupies about eight days, which is sufficiently long to afford the advantages of a short sea voyage. The equable temperature of the air at sea, its purity, the saline matter it contains, and the less variable pressure of the atmosphere, all co-operate, with the gentle unceasing motion of the ship, and the slight nausea generally experienced during the first few days on board, to bring the patient into a proper condition before he lands for receiving all the benefits derivable from the climate of Malaga.

The city of Malaga, one of the chief towns of Andalusia, is situated in a deep and beautiful bay, surrounded by a fertile valley opening to the south on the Mediterranean, and protected on the west, north, and east by the lofty mountains of Ronda, Antequera, and the Sierra Nevada. Some idea of the altitude of these ranges, and of the purity of the atmosphere, may be gathered from the fact, that from the Granada road, immediately behind the town, the view embraces the vast extent from Gibraltar, on the west, to Velez Malaga on the east, and across the Mediterranean to where the summits of the African Atlas are distinctly visible to the far south. I shall not here enter on any general description of the city of Malaga, but shall at once pass to what more directly interests medical inquirers, *i.e.*, the climate and topography of the place, and their influence on health and disease.

The latitude of Malaga should render its climate warmer than any of the health resorts of the south of France or Italy, as it lies 410 miles south of Pau, 486 south of Nice, 250 south of Naples, and upwards of 300 South of Rome. The climate of Malaga belongs to the class of climates described by Dr. Corrigan under the name of *tonic*. It is remarkably dry and equable, warm, and somewhat bracing. So steady is the temperature of the day that, according to my observations, the mean daily variation of the month of December was only $2\frac{1}{2}^\circ$ (Fahr.); the greatest daily variation during that month was 5°, and the smallest was 2°. In January the mean daily variation was 3°; the maximum 9°, and the minimum 2°. In February the average daily variation was $2\frac{1}{2}^\circ$; the greatest 7°, and the smallest 2°. But, though the temperature of the *day* in Malaga is, perhaps, the most equable in Europe, yet the temperature of the twenty-four hours of day and night is not by any means so, for, immediately after sunset, there is a remarkably sudden and very perceptible diminution of temperature, frequently accompanied by a profuse fall of dew, rendering the atmosphere humid and chilly. It is, therefore, highly important to apprize invalids going

to Malaga of this fact, that they may avoid the great injury which results to those who adopt the common practice of strolling on the Alameda after sunset.

The mean annual temperature of Malaga is 65°, or 16° higher than Dublin, 9° higher than Pau, 1° lower than Algiers, and 7° lower than Cairo. The mean temperature of winter is 55°, or 13° higher than Dublin, also 13° higher than Pau, exactly the same as Algiers, and 3° lower than Cairo. In Spring the mean temperature is 68°, or 24° higher than Dublin, 14° higher than Pau, 2° higher than Algiers, and 3° lower than Cairo. The temperature of summer and autumn need not be considered, as Malaga is only resorted to by invalids during winter and spring. The last winter I spent in Malaga was a fair average season, and during the month of December the mean temperature of the day was 59½°; the highest temperature I observed was 68°, and the lowest 52°. In January the mean daily temperature was 57°; the highest 61°, and the lowest 50°. In February the mean temperature of the first fourteen days was 58°; the highest 66°, and the lowest 50°.

Rainy days are less numerous in Malaga than in any part of Southern Europe frequented by invalids for change of climate. Thus, on an average of ten years, there are only 29 rainy days annually in Malaga, or 90 days less than in Pau, 88 less than Rome, 45 less than Nice, 44 less than Madeira, and 41 less than Algiers. During the year 1863 the number of days on which rain fell was greater than the average, amounting to 38.

The annual rain-fall is also comparatively small, being about 16½ inches. It is generally of tropical character, falling in large drops, and with considerable force, and is therefore of short continuance. The most rainy month is February; and during the three Februaries I passed in Malaga invalids were confined to their rooms for fully half the month by damp, and even cool weather.

I am indebted to Mr. John O'Kelly, of the Geological Survey of Ireland, for the following very valuable tables of his meteorological observations in Malaga, during the winter of 1863–64:—

Meteorological Observations, Malaga, November, 1863.

Day of Month	Attached thermometer 9 am.	Rain	WIND		Barometer reduced to 32° at sea level	Remarks
			Velocity	Direction		
1	..	..	..	..	..	
2	..	..	..	..	..	
3	..	..	..	..	..	
4	70	None	..	..	30·40	
5	70	,,	..	..	30·30	
6	68	,,	..	..	30·34	
7	68	,,	..	..	30·14	
8	70	,,	..	..	30·14	
9	70	,,	Light	N.W.	29·90	On the 9th the thermometer exposed registered 114° in the sun, and 72° in the shade.
10	69	,,	,,	,,	29·90	
11	68	,,	Fresh	,,	30·00	
12	68	,,	Rough	,,	29·84	
13	66	,,	,,	,,	29·84	
14	66	,,	Light	S.W.	30·06	
15	65	,,	,,	,,	30·15	
16	65	,,	,,	S.E.	30·18	
17	65	,,	,,	S.S.E.	30·12	
18	65	,,	Fresh	E.S.E.	30·12	
19	66	,,	Calm	—	30·24	
20	66	,,	Fresh	S.E.	30·26	
21	66	,,	,,	E.S.E.	30·24	
22	66	,,	,,	E.	30·12	
23	66	,,	Light	E.S.E.	30·00	
24	66	,,	,,	E.S.E.	30·00	
25	66	,,	,,	S.E.	30·06	
26	68	,,	Fresh	,,	30·10	
27	66	,,	,,	E.S.E.	30·02	Exposed thermometer, in shade, 68°.
28	68	,,	,,	,,	29·90	
29	68	Rain and storm in morning	Evening calm	E.S.E.	29·79	
30	68	None	Light	S.E.	29·92	

Meteorological Observations, Malaga, December, 1863.

Day of Month	Attached Thermometer	Exposed Thermometer in shade	Rain	WINDS		Barometer reduced to 39° at sea level	Remarks
				Velocity	Direction		
1	70	..	None	Light	E.S.E	30·02	Exposed thermometer at 10 p.m. 62° at 11 a.m.; 98° in sun.
2	69	..	,,	,,	W.N.W.	30·20	
3	70	..	,,	,,	N.N.W.	30·22	
4	71	..	,,	,,	S.E.	30·24	Barometer at 8 p.m., 30·38.
5	70	..	,,	,,	S.S.E.	30·32	
6	70	..	,,	,,	S.E.	30·34	
7	69	..	,,	,,	N.E.	30·26	
8	68	..	,,	Calm	N.	30·25	
9	67	..	,,	Light	E.S.E.	30·32	
10	64	..	,,	,,	,,	30·34	Exposed thermometer at 9 a.m., 60°; at 6 p.m., 60°.
11	64	..	,,	,,	,,	30·38	
12	68	60	,,	,,	S.S.E.	30·38	
13	68	60	,,	Calm	S.	30·20	Exposed thermometer 60° at 6 p.m.
14	68	57	,,	,,	S.	30·22	
15	68	60	,,	Light	S.E.	30·22	
16	68	60	..	,,	Shifting, N. to S.	30·21	Cloudy in morning.
17	66	60	..	Fresh	N.W.	29·94	
18	64	56	Some rain, cloudy	,,	N.W.	29·66	
19	64	54	..	Light	Shifting, N. to S.	29·82	
20	60	52	..	Calm	..	29·91	
21	58	52	Cloudy	,,	..	30·02	
22	58	54	Bright	Nearly calm	S.W.	30·10	
23	62	54	,,	,,	N.W.	30·02	
24	65	58	,,	,,	..	30·18	
25	65	58	,,	Calm	..	30·16	
26	65	56	,,	..	..	30·14	
27	65	58	,,	Fresh	N.W.	30·10	
28	65	58	,,	,,	Shifting, N.W. to S.E.	30·22	Wind N.W. in morning; shifted to E.S.E. in evening.
29	64	58	,,	Light	S.E.	30·24	
30	65	58	,,	,,	S.S.E.	30·20	
31	65	..	,,	Fresh	N.W.	30·02	Terràl.

Meteorological Observations, Malaga, January, 1864.

Day of Month	Attached Thermometer	Exposed Thermometer in shade	Rain	WINDS		Barometer reduced to 32° at sea level	Sky	Remarks
				Velocity	Direction			
1	65	57	None	Rough	N.W.	30·02	..	Terral.
2	65	58	„	Fresh	W.	30·04	..	
3	65	60	„	Calm	„	29·96	..	Thermometer in sun, at noon, 88°.
4	62	52	„	Rough	E.	29·96	..	Stormy at night.
5	60	52	„	„	E.	30·02	Cloudy	Cold wind to-day.
6	59	52	„	Light	„	29·92	„	
7	59	52	„	Rough	S.E.	29·89	„	Stormy at night.
8	62	54	„	Fresh	S.E.	29·69	Bright	
9	62	..	Rain	Light	„	29·84	Cloudy	
10	62	..	Rain	„	S.	29·80	Cloudy	
11	64	..	None	Fresh	W.	30·04	Bright	
12	64	..	Rain at night	Nearly calm	S.	30·22	Morning fine	
13	62	..	Rain	„	S.E.	30·09	Cloudy	
14	63	..	None	Fresh	W.S.W.	30·12	Bright	
15	62	..	„	„	E.	30·21	Cloudy	
16	67	..	„	Light	E.	30·18	Bright	
17	65	..	„	„	E.S.E.	30·00	„	
18	65	..	Rain in morning	Fresh	E. to N.W.	30·00	Cloudy	
19	67	..	None	Light	E.	30·22	Bright	
20	63	..	„	Rough	E.	30·32	Cloudy	
21	62	..	„	Light	„	30·30	Cloudy	
22	63	..	„	„	„	30·30	Bright	
23	60	..	„	„	„	30·32	Cloudy	
24	64	..	„	„	„	30·30	Bright	
25	61	..	„	Fresh	„	30·22	Cloudy	
26	66	..	„	Light	E.S.E.	30·32	Bright	
27	61	..	„	„	E.	30·30	Cloudy	
28	64	..	„	„	E.N.E.	30·22	„	
29	66	..	„	Fresh	S.W.	39·02	Bright	
30	64	..	Rain in morning	..	„	30·02	Cloudy	
31	62	..	Some rain at night	Fresh	E.	30·12	Bright	

Meteorological Observations, Malaga, February, 1864.

Day of Month	Attached Thermometer	Rain	WINDS		Barometer reduced to 32° at sea level	Sky	Remarks
			Velocity	Direction			
1	64	Rain in morning	Generally fresh	E.S.E.	30·12	Bright	No rain fell from 8 a.m.
2	62	None	„	„	30·30	„	
3	62	„	Light	S.E.	30·32	„	
4	64	„	„	E.S.E.	30·20	„	
5	64	„	Fresh	E.N.E.	30·00	„	Sharp wind, snow on hills.
6	60	„	„	E. to N.	29·94	„	Ditto
7	60	„	„	„	30·02	„	Ditto
8	60	Rain in evening	„	N.E.	30·00	Cloudy	Ditto
9	60	Rain in morning	Light	„	29·64	„	
10	62	None	Fresh	N.E. toS.E.	29·90	Bright	
11	62	„	„	N.	29·84	„	
12	66	„	Light	S.E.	30·60	„	
13	66	„	„	S.W.	30·24	„	
14	69	„	„	„	30·32	„	
15	69	„	„	S.S.W	30·22	„	
16	65	„	„	N.W.	30·20	„	
17	64	„	„	N.W.	30·04	„	
18	64	„	„	„	29·74	„	
19	65	„	Fresh	„	29·54	„	Barometer, 7 p.m., 29·42.
20	66	„	„	E.	29·42	„	
21	65	„	„	E.	29·50	„	
22	60	Rain	Rough	E.	29·60	Clouds	
23	60	„	„	E.S.E.	29·72	„	
24	60	„	Light	„	29·80	„	
25	64	„	„	„	29·84	„	
26	63	Showers	„	„	29·90	„	Exposed thermometer, at 12 o'clock, 71°.
27	64	„	Calm	„	29·78	„	
28	63	Rain	„	„	29·70	„	
29	64	Fine	Light	S.W.	30·01	Bright, some clouds	

Meteorological Observations, Malaga, March, 1864.

Day of Month	Attached Thermometer	Rain	WINDS		Barometer reduced to 32° at sea level	Sky	Remarks
			Velocity	Direction			
1	64	None	Light	S.E.	30·00	Bright	
2	63	„	Calm	E.S.E.	29·98	„	Very fine.
3	63	„	Light	„	29·00	„	
4	65	„	Calm	„	29·66	„	
5	65	Rain	Rough	S.W.	29·40	Clouds	Very wet; barometer, 10 p.m., 29·86.
6	66	None	Light	„	30·00	Bright	Very fine.
7	66	„	Fresh	S.S.E.	29·99	„	
8	68	Showers	„	S.E.	29·80	Some clouds	
9	68	Rain in morning	„	„	29·74	Clouds	

The mountains beyond the Vega almost completely shelter the town from the north and west winds. The prevailing winds are the east, or *levante* which is cold and humid in winter, the north-west and the south-east. The following table shows the

Frequency of the various Winds during the year 1863.

Winds	Number of times they occurred	Remarks
North to east, .	140	These observations were taken three times a day.
East to south, .	460	
South to west, .	143	
West to north, .	352	

The north-west wind is termed *un viento fatal*, a deadly wind, by the natives. It rushes through a gap in the Antequera mountains, called the *Boca del asno*, along the valley of the Guadalmedina, and arrives in Malaga laden with fine sand, which irritates the pulmonary mucous membrane. In summer this wind is hot and dry, giving rise to a highly irritable and nervous state of mind and body. It is a curious illustration of this, that the cases of stabbing, for which Malaga is unfortunately so notorious, bear each year a constant proportion to the number of days the *terral* has prevailed. In winter the north-west wind is still a dry wind, but, sweeping down the snowy mountains, is then intensely cold. Its ill effect,

however, cannot be measured by the thermometer alone; for the force and rapidity of its motion, its aridity, and the quantity of impalpable sand it suspends, all combine with its low temperature to injure the valetudinarian.

We must also bear in mind that the *terral* suddenly takes the place of the warm and genial air that usually prevails in Malaga; and thus produces all the ill effects of a great, rapid, and unprovided-against change of climate. While the *terral* lasts, the hands and lips become chapped, a tendency to epistaxis is produced, cough and expectoration are increased, and changed for the worse; and, in consumptive cases, hemoptysis is frequently brought on. The general health, too, suffers, and, the mind sympathising with the body, the patient becomes irritable and despondent. It is, therefore, essential to impress on the patients we send to Malaga the necessity of remaining within doors as long as this wind prevails, which, fortunately, is seldom for more than three consecutive days. This advice is the more important as all foreign invalids in Malaga reside on the Alameda, or public promenade, which is fully exposed to the *terral.*

The last wind I need speak of is the South-east, which in its passage from the opposite African coast, over eighty miles of sea, absorbs a good deal of moisture, and in winter is regarded as mild and healthy. In summer, however, it resembles the Sirocco (the *plumbeus Auster*) of the Italian coast in its effects, and occasions the well-known feelings of malaise and depression which that wind produces.

According to Senor Montes, the barometer is very steady in Malaga; having taken no notes on the point I am unable either to corroborate or dispute this statement. I am, however, indebted to Mr. O'Kelly, of Rochestown, for the following table, kept by a resident observer, showing—

The State of the Barometer during the year 1863.

Mean height of barometer at 9 a.m., . .	29° 1·7″
,, ,, ,, 3 p.m., . .	29° 1·
,, ,, ,, 11 p.m., . .	29° 1·4″
,, ,, ,, during the year, .	29° 1·4″
Maximum, ...	29° 8·5″
Minimum, ...	28° 2·9″

Snow falls very rarely in Malaga; so seldom, indeed, that when

Senor Martinez published his *Topografica Medica de la Cuidad de Malaga*, in 1852, he states (p. 145) that there was no record of its ever having snowed within the city. But since then a small quantity of semi-fluid snow fell one day in February, 1860, to the great astonishment of the Malaguenans. It must be borne in mind, however, that although cold weather occurs but seldom in Malaga, and lasts for only a very short time, yet it is far more severely felt there than it is in this country. For in Malaga the houses are built with the intention of excluding the certain and intense heat of summer, and no precautions are taken against the less certain cold of winter. In the hotels and lodging-houses the rooms are large and sparingly furnished, there are neither carpets nor window curtains; the only means of warming the apartments is a *brasero*, or pan of charcoal, which is a poor substitute for the cheerful blaze of an open fire; and as the smouldering charcoal, of course, evolves carbonic acid gas, is dangerous as well as unpleasant.

I believe that the benefit which results from sending our consumptive patients to southern climates is in a great measure owing to the fact that in such climates invalids may pass most of their time in the open air, and take far more out-of-door exercise than they could in this country. For example, in 1863, the following was the state of the weather and atmosphere at Malaga—

Fine bright days,	213
Cloudy,	75
Foggy,	34
Days on which some rain fell,	27
Wet days,	11
Stormy,	5

While in Dublin, at the same time, there were 176 days on which the sky was overcast, and 212 days on which rain or snow fell.

With respect to the living in Malaga, which is a very important point, as it is essential that the little sustenance an invalid takes should be nutritious and digestible; it must be admitted that in both respects the animal food in Malaga is very inferior to that used in this country. The meat there is generally hard, stringy, lean, and flavourless, except the pork, which, as throughout Spain, is excellent, but is not a fit food for invalids. The fish, however, is of such good quality, and of so many various kinds, that it makes

up in a great measure for the shortcomings of the animal food, and invalids may manage to live very well and very cheaply at the hotels, on fish, kid, turkeys, ducks, fowl, game, especially partridge, which is a standing dish here, and vegetables and fruits, many of which are unknown, except as rarities in this country, even if they do not choose to venture on the meats served at the table d'hote.

Spanish cookery is generally considered as intolerable by British travellers, but I think this is mere prejudice; and that garlic and oil, which enter so largely into all culinary operations in Spain, are absolutely necessary (moderately used) to supply the want of fat and of flavour in the meat, and to render it more digestible.

The physician who sends his patient to Malaga should impress on him the great importance of selecting an apartment having a southern aspect, as there is often a difference of ten degrees in the temperature of rooms facing the Alameda and those at the back of the hotels. Besides the mere warmth, the front rooms are more cheerful, and enjoy the advantage of free exposure to the light and sun, a very essential matter for a pulmonary invalid, who should recollect the Italian proverb, that—*Dove il sole non entra il medico viene*, "where the sun does not enter, the doctor must."

The hygienic condition of Malaga is as defective as it can well be. In a great many of the houses there is no provision for sewerage of any kind; and even in the more civilized part of the city, in the hotels on the Alameda, the drainage is very bad indeed. The main sewers, which run under the principal street, are choked up by the decomposing accumulation of years, and being provided with immense square openings, through which the dirt and rubbish is thrown into them, in the centre of the streets, the mephitic gases evolved below freely escape into the atmosphere of the narrow lanes of the city. The bed of the Guadalmedina is really the main sewer of Malaga; and as for nearly ten months annually it is little more than a wide dry bed of gravel, being dependant on the torrents in winter for its purification, the odour it exhales in warm weather renders a residence near it as disagreeable as it is unhealthy.

The connexion between epidemic disease and bad sewerage is, I think, very well illustrated in Malaga, which has at all times been remarkable for the prevalance of zymotic diseases. I have collected from the older Spanish writers notices of no less than twenty-two epidemic pestilences, some of which almost depopulated the city between 1493 and 1804. The earlier of these seem to have

been epidemics of genuine oriental plague, and the latter generally assumed the form of yellow fever. Of late years, since 1834, these pestilences have not appeared, but their place has been taken by Asiatic cholera, which has several times ravaged the town.

From the returns of the civil hospital of Malaga we learn that the most unhealthy month there is January, and that, strange as it seems, less sickness occurs in December than at any other time The most prevalent diseases are the eruptive fevers, and especially variola; intermittent and bilious fevers, acute affections of the respiratory organs, such as catarrh, bronchitis, pleurisy, and pneumonia, the latter being often conjoined. Acute and chronic maladies of the gastro-intestinal organs also prevail extensively. Diseases of the eyes are very common here, and I know of no country, except Egypt, in which one meets with more blind people. That rare and interesting malady elephantiasis arabum is occasionally witnessed in this town; and during one year no less than seven cases of it were admitted into the civil hospital. Some of these cases which I saw in that institution were as well marked instances of elephantiasis as I ever met with in any part of Africa I have visited.

In my work on change of climate, published in 1864, was given, I believe, the first account of a peculiar affection which frequently attacks strangers within a few weeks of their arrival in Malaga. It consists of a very painful acute inflammation of the margin of the gums, leading to the formation of minute ulcers round the teeth, and attended with considerable constitutional irritation. The natives attribute it to some article of the diet, but I think we may ascribe it to the Catalan wine ordinarily used in the hotels. This wine which is strong and acrid, and slightly acid, leaves a considerable deposit of tartar in the vessels in which it is kept even for a few days—and I think it is not improbable may act as an irritant on the mucous membrane of the gums. The treatment consists in stimulants and astringent gargles; low diet, and cooling laxatives are required, and I have sometimes been obliged, after the failure of other means, to employ freely a strong solution of nitrate of silver to the ulcers, notwithstanding its disagreeable effect on the teeth.

Senor Martinez Y. Montes, Physician-in-Chief to the Military Hospital, has collected tables from which we find that in January the number of deaths was larger than in any other month, while in May it was smaller. The total number of adult deaths during

the year was 9,049. The greatest mortality from any one disease was 711 from dropsy; and the smallest was one death from hydrophobia. Acute and chronic diseases of the respiratory organs, not including consumption, were the cause of 1,208 deaths. The mortality from phthisis was 407, more than half of which, or 234, occurred in the civil hospital. Cerebral affections are apparently a prolific source of mortality here; 742 fatal cases of these complaints occurred, of which no less than 407 are set down to apoplexy.

In Malaga the deaths occasioned by consumption, among the native population, are less numerous than in any other European southern locality resorted to by invalids in winter. This is, I think, a very significant fact; for, as we might naturally consider a place where the mortality from phthisis was very great an unsuitable residence for phthisical invalids, so, a city like Malaga, where the mortality from that disease is remarkably small, should be a favourable locality for such patients. Its superiority in this respect to our own climate is sufficiently proved by the fact, that in Ireland 125 deaths out of every thousand are caused by consumption, while in Malaga only 34 deaths per thousand result from that disease.

It is a curious circumstance, however, that the mortality from phthisis in Malaga is becoming notably greater each year; and though, of course, some part of this increase is owing to the growth of the population, and a still larger proportion is occasioned by the deaths of foreign invalids, yet a considerable number of phthisical deaths remain which cannot be thus accounted for. Hence a belief in the infectious nature of consumption is very generally entertained here, and many of the lodging-house proprietors object to receive phthisical invalids. I may here observe that the belief in the communicable character of consumption prevails very extensively in Southern Europe, especially in the south of Spain and Italy, and particularly in Naples. This opinion, though it may be traced back to many of the old writers, will now find few supporters in this country. I cannot help thinking, however, that, in hot climates at least, there is a good deal of truth in it; and that constant communication with consumptive patients, such as sleeping in the same room with them, and breathing an atmosphere charged with the morbid exhalations from their diseased lungs is very likely to prove injurious to persons in delicate health, and may even determine the occurrence of phthisis in individuals who might otherwise have escaped this malady.

When we come to consider the mortality from all chronic diseases of the air passages, we find the superiority of the climate we are discussing is not so conspicuous as it might at first appear; for the deaths in Malaga from all chronic affections of the respiratory organs are very nearly as great as in Dublin, amounting, according to Senor Martinez Y. Montes, to nearly one-ninth the entire number recorded.

The dry, warm climate of Malaga is occasionally of great service to children hereditarily predisposed to tubercular diseases; and also to young persons in whose constitution a scrofulous taint is lurking, which if it does not affect the glandular system, may, about the age of puberty, pass into the premonitory symtoms of consumption.

In cases of tuberculous cachexia, and in that state of pulmonary irritation which always precedes the deposit of tubercle in the lungs, sometimes described as the "pretubercular stage of phthisis," the climate of Malaga produces its most decided curative result. Such cases may often be permenantly cured by a sufficiently protracted winter residence in this climate for several successive years; during which the same precautions must be observed, and the same regimen and medicinal treatment be persevered in which would have been necessary had the case been treated at home. When the first stage of the disease has developed itself, and when a small amount of tubercular deposit has taken place, the case, though less under the influence of the climate, is still so to a great extent, and the foregoing observations are applicable to it. When the malady has advanced a step further, and some softening of the tubercles has occurred, then the chance of perfect recovery is of course greatly lessened; but the progress of the disease may be still arrested, and may be so long staved off that the patient may ultimately die from some other cause. To accomplish this end he must, however, live constantly under medical rule and in a suitable climate; and there is probably no European climate more proper for the purpose than that of Malaga.

It has been asserted by a very eminent authority that when a cavity exists in the lungs—"a southern climate is not only useless but injurious." I have, however, seen patients who when they landed in Malaga could hardly walk from the jetty to their hotel on the Alameda, so weakened were they by hemoptosis, increasing cough, night sweats, and purulent expectoration, and who presented the physical signs, as well as the symptoms, of a cavity, and yet in a few weeks were so improved that they could ride some miles to

a pic-nic, enjoy themselves there, and return home, apparently none the worse for the fatigue and the excitement. When the symptoms were so mitigated the patient would generally imagine that his disease had yielded to the climate, and would, with the characteristic hopefulness of the complaint, indulge in distant expectations and plans for the future. But, in most cases indeed, the improvement was only temporary, the symptoms slowly returned, and the fatal event was not prevented, but only postponed. Any prolongation of life, however, is considered so great a boon that a measure like this which affords a hope of adding to a life which must be shortened by consumption if unchecked, and even offers a possible chance, however small, of curing the disease itself, must be considered as an important addition to our mode of treatment. I think that very few phthiscal patients are sent to Malaga who do not require cod-liver oil; but unfortunately many invalids as soon as they arrive there think that the climate alone should effect the cure, and lay aside the remedies they had been taking at home with advantage. Patients should, therefore, be warned against such an error; and, as the medicines sold in Malaga are seldom good, they should bring a small stock of this oil, a little tannin, and whatever else the physician considers necessary along with them. Cod-liver oil is, I believe, even more necessary in Malaga, where the meat is dry, lean, and innutritious, than in this country where it is the contrary.

Asthmatic patients often derive singular benefit from the climate of Malaga. But this climate cannot of course be applied indiscriminately to all cases, for it is obvious that an atmosphere which would serve a case of dry asthma must be injurious to the humoral form of the complaint. For instance, the moist warm air of Pisa, which would probably give marked relief in a case of spasmodic nervous asthma, would only increase the sufferings of an invalid labouring under the other form of the disease.

Chronic bronchitis is another malady on which change of climate exercises a very sensible influence, and it is often surprising how slight a change of air will cure chronic bronchitis that has resisted all the ordinary remedies. Within my own experience I have six or seven times seen a change from Dublin to the south side of Dalkey or Killiney, not ten miles distant, followed by immediate relief of a harassing bronchitic cough, which had been previously treated in the usual manner without benefit. But in more confirmed cases, the change of climate must be great and complete to do good, and a winter in Malaga sometimes agrees admirably with

elderly patients suffering from this complaint. In some instances, however, of a more irritative form of the disease, I have seen a fresh attack of acute bronchitis brought on by this climate, and one case that I attended resulted fatally from that cause.

It is quite as important to know what patients should avoid any climate as to know what class should select it. Now, considering the great mortality from cerebral affections, and particularly apoplexy, in Malaga, I think that patients predisposed to such diseases should not choose this town for their residence. Nor would I send a patient suffering from chronic rheumatism, or rheumatic arthritis, or neuralgia, to Malaga; as the great difference between the temperature of the day and night, and the heavy dews that fall after sunset, render this town in such cases inferior to other climates, such, for instance, as Western Australia, Upper Egypt, and in some cases Nice. Nor can Malaga be advised to dyspeptic and hypochondriacal invalids as the dietary there is not generally suitable for these cases.

The climate of Malaga was considered by the older native writers, Don Fernandez Barea, Padre Garcia de la Lena, and others, to exert a relaxing and unfavourable influence on the constitution of young persons under the age of puberty. But they regarded this locality as an advantageous residence for the old, and thought the climate a propitious one in which to "husband out life's taper at its close," and calculated, by aiding in the alleviation of the many physical annoyances of the aged, to add to the span of their existence.

In conclusion, I have only to express my opinion, that when a very dry, warm, tonic winter climate is indicated in the treatment of chronic pulmonary disease, especially phthisis, Malaga will be found superior to any health resort in Europe, and only inferior to Western Australia and Upper Egypt.

ART. III —*Remarks on Hydrocephalus; its Diagnosis, Prognosis, and Treatment.* By HENRY KENNEDY, A.B., M.B.; Fellow and Censor of the King and Queen's College of Physicians; one of the Physicians of the Cork-street Hospital.

[Read at the Dublin Obstetrical Society, 11th February, 1865.]

TWICE already the attention of this society was directed to the subject of hydrocephalus. On the first occasion I spoke of a form of the disease which was peculiar in this respect—that it occurred

at a more advanced period of life than the more common disease, being met between eighteen and twenty-five years of age—that it was a disease of a very insidious character, and almost necessarily fatal. As the paper is in print, more need not be said about it here. In the second paper some cases of the disease, as met in childhood, were detailed, in which recovery took place under very unpromising circumstances. It is this last phase of the subject I would again bring under notice; for it must be allowed that hydrocephalus is a disease entailing a great mortality. I recollect perfectly well, some twenty years ago, regarding the disease as all but hopeless. I now believe that, though it must still be regarded as a very serious affection, it may yet be looked on in a much more favourable aspect, inasmuch as recoveries are not uncommon, and, what is of more consequence, are directly due to treatment.

Of the nature of this affection there would seem now to be little doubt; and all writers are, I believe, at present agreed, that it is essentially found in the strumous constitution. Cases may, and I know do, occur where the evidences of such a state are wanting; but these are very exceptional, and only prove the rule. Nor is it a trivial matter to have a correct idea of the kind of constitution in which hydrocephalus occurs, for on this turns—necessarily turns—a great deal relating directly to the diagnosis, prognosis, and treatment of the disease. It has been my lot to examine more than twenty cases after death, and in all, besides the mischief in the brain, other organs, as the lungs, liver, and spleen, were likewise diseased more or less by the deposition of tubercle, as it is usually met in childhood. With such evidence I never meet a case of hydrocephalus now that I do not consider and treat as if it were a strumous constitution with which I had to deal. I have stated that the prognosis is modified by the views we take of the nature of this disease, and probably a few remaks on it, as well as the diagnosis, will not be considered out of place.

The diagnosis of this disease, it will be allowed, is often sufficiently puzzling. I do not speak of it when it is fully formed, and it is scarcely possible to mistake it, but at an earlier stage, when it is of every consequence to recognize it. I presume I address few who have not met cases of worm, or gastric fever, as it is called, which have suddenly changed their character—symptoms of water on the brain coming on, and so ending life. The same remark, too, may be made of the other fevers of childhood, such as scarlatina, measles, and hooping-cough, if the latter be a fever—in each and

all of which I have seen the disease supervene, and thus bring to a fatal termination cases which had begun with comparatively mild symptoms. Some of these cases have happened to myself, and more suddenly than was at all desirable, and I know they have occurred to others. Hence the greater need of caution, and being ever on our guard; and were I asked whether these sudden changes occurred in cases commencing with mild or heavy fever, I would at once answer the former.

Now the question may here be asked—why is it that such cases occur, or seem to occur, suddenly, and have we any means of foreseeing them? My answer is, that hydrocephalus is just like other affections in this respect, that its symptoms vary, and some of the more striking of them may be entirely absent, or so little marked as to escape notice. The impression with some is that every case must have vomiting; but, general though this sign be, it is not by any means constant; and anyone familiar with the morbid anatomy found in the brain in these cases, will see at once why it is not always present, for the same parts of the organ are not constantly engaged, nor the origin of the eighth pair necessarily involved. Hence it is that, just as may occur in pneumonia or pericarditis, hydrocephalus may likewise be more or less latent as regards its symptoms, and so may steal a march on us.

In helping us to a correct diagnosis—and I am, of course, still speaking of cases where the disease is obscure—it has appeared to me that much may be gained by learning what goes on during the night. All diseases are liable to exacerbations at this period of the day; but, in addition, we know that sleep is induced, in part at least, by an altered state of the circulation in the brain as contrasted with the waking hours. Hence a disease like hydrocephalus is very apt indeed to be attended by symptoms which show themselves, in the first instance, at night. Whether the explanation be correct or not, certain it is that a knowledge of what goes on during these twelve hours will often materially aid us in arriving at a sure diagnosis, and, if I may so say, foreseeing what is about to occur—a matter of no little moment to us all.

Into the general symptoms of the disease it would not be possible here to enter. They have been ably described by many, though possibly by none more so than by the late Dr. Cheyne, whose admirable work must even yet be considered a standard one on this disease. There are, however, two or three points worthy of a passing remark. Thus, in the class of cases of which I already

spoke, where the disease seemed to commence as gastric or worm fever, I have had occasion often to observe, when head symptoms threatened, a marked change to occur in its type. The pulse has become, in a marked degree, stronger, the skin hotter, and the tongue still whiter. In fact the fever has become much more of the inflammatory type; and this, let it be observed, before any complaint was made of the head. So that such an event occurring should at once put us on our guard.

But again: whilst we know that pain in the head is a very constant symptom of the disease, it is not always present, and I have seen several cases where the nape of the neck was the part of which the child first complained—cases, I mean, which ultimately ended in water on the brain. This is a point of some consequence, as it shows the part of the brain about being attacked, that is, close to the pons and upper portion of the spinal marrow; and besides, I believe our treatment cannot be applied too close to the seat of mischief. It is in these cases, when pain is referred to the neck, that we so often observe the child lying with the head drawn back, and more or less fixed—a state which, I am sure, all present must have witnessed.

Before leaving the subject of the diagnosis of hydrocephalus, I would notice a state, which if not the disease itself, is so very like it as to be worthy of remark here. It is a state with which I have been long familiar; but my recent connexion with the Cork-street Hospital has brought it much more prominently before me. What I allude to is a mixture of typhus fever, as shown in most of them by the presence of spots, with very marked brain symptoms. At certain periods there has been a run of cases of this kind which is quite remarkable. A sketch of two or three of them will illustrate the disease better than any mere description.

Case. I.—Fitzgerald, aged seven years, admitted in March, 1853, with the heaviest fever a child could exhibit. The tongue was dry as a board, and the lips covered with sordes. With this state he had every symptom of water on the brain, except vomiting. He had dilated pupils—one more than the other; sighing; grinding of the teeth; squinting. Finally, convulsions and death.

Case II.—In July, 1855, a boy of ten years of age was sent into Sir P. Dun's Hospital by Dr. Macready. When I first saw him he had very heavy fever, and was spotted. He was quite

stupid, and hard to rouse; had a marked frown on his forehead, and any complaint he made was of his head. His pupils were both dilated; pulse rapid; discharges from the bowels blackish; and every now and then he had the cry so characteristic of hydrocephalus. But he had no vomiting, nor grinding of the teeth. For my present purpose it is enough to say this boy recovered, but very slowly—the pupils gradually coming round to their normal state.

Case III.—Coleman, aged ten years, admitted to hospital in November, 1855. He had the regular typhus, and was spotted. His tongue was too clean; whilst there was oozing of blood from his lips and gums. Two days later signs of the brain becoming engaged appeared. He had severe pain in the head, and marked screaming; whilst the pulse, previously rapid, fell to 60. From this state he very slowly emerged. There was not, either, in this instance, any vomiting. Other cases very similar to these could be given, but what have been detailed are quite sufficient for my present purpose. They show that in the progress of spotted fever brain symptoms having a very close resemblance, I do not say identity, with hydrocephalus may arise. They differ, however, from the more regular disease in not being at all so fatal; for, though they are always serious, the majority recover, which cannot be said of the other form of brain affection. It has appeared to me that the typhus poison, which was unquestionably present, as shown by the spots, caused such a revolution in the system as to overcome the tendency to engender hydrocephalus, for that there was such a tendency appears to my mind to admit of no question. But inviting though this part of my subject is, I must hasten on.

And now, supposing the diagnosis of hydrocephalus made, what is to be the prognosis? Must it necessarily be fatal? or are there any reasons why, in some instances at least, better hopes may exist? I believe there are; and for my own part cannot doubt that I have now seen several cases, where the disease was well marked, recover, and before concluding this paper shall detail some of them. If, then, there be such cases, and for the present I shall assume there are, it will be well we should be able to recognize them, as on such knowledge it is our prognosis must obviously be founded. The points, then, that appear to me of every consequence in forming our judgment are mainly two. The first, and by far the most important, is the previous history of the child, for the longer the

attack has been coming on the more serious will be the result—I mean before the brain symptoms declare themselves. It will be recollected from what has preceded that we must consider the disease as essentially a constitutional one; and though I cannot state it as a positive fact from *post mortem* examination, I have got a strong conviction that in many cases mischief elsewhere has preceded the appearance of the cerebral affection. In other words, that the latter is but the finale of a state of disease which had been going on long before the brain became engaged. In this view of the matter it will be understood how important it is to get a thorough knowledge of the antecedents of the case, and every point, no matter how trivial, should be most minutely investigated. For myself, I believe that this knowledge is of very much more consequence in forming a correct prognosis than any present symptoms which the patient might present. As, however, it is only the general principle I speak of here, it will not be possible to enter into particulars, further than to say that loss of flesh and appetite, wandering pains through the body, derangement of the bowels, and, above all, the slightest evidence of cough, are amongst the most important symptoms for which we should enquire. Even in hospital cases, where it frequently happens we can get no satisfactory account of the previous state of the patient, a single glance reveals much. It tells us the shape of the chest, and general conformation of the body, besides all those marks which need not here be detailed, but which betoken the strumous diathesis, for it cannot be too strongly impressed on our minds that this it is which makes hydrocephalus such a formidable disease. As a corollary, then, from what has been just stated, it may, I believe, be taken for granted that if we meet the disease in a child otherwise healthy, and with none of those precursory symptoms at which I have glanced, we may have more hopes of the final result, and modify our prognosis accordingly.

In the last place, I would make a few remarks on the treatment of this disease. This, however, will be best done after giving the briefest detail of a few cases, each of which presented some feature of interest. The following is the only case I have seen of what may be called acute arachnitis of the brain occurring in childhood, and I introduce it here as affording a marked contrast to the cases which follow it, and as bearing out some of my previous remarks. I saw the case many years back with my friend, Dr. Faussett, now of Clontarf:—

Case IV.—A very fine-made boy, of four years of age, was reported to have got a severe fall, a few days subsequent to which he began to complain of pain in his head. This increased rapidly, and was shortly followed by very severe vomiting. He had been dull and listless for a day or two before he made any complaint of his head. The very active treatment used had but a temporary effect in checking the symptoms, and at the end of the second day from the time the vomiting set in, convulsions came on, and the boy died on the fourth day. Examination disclosed a quantity of pure pus, chiefly on the anterior lobes of the brain, rather more being on one side than the other. A number of bloody clots were observed on cutting into the substance of the organ. Some turbid fluid was found in the ventricles.

Case V.—A boy of eleven years of age was admitted into hospital, after, as it was stated, being a week ill. He had not been treated in any way, and presented all the signs of hydrocephalus in the second stage—his pupils being much dilated, the left the most, and his pulse being only sixty-four. The case passed into the third stage, and the boy ultimately died. But I mention it here because it was the first case I had seen where there was even a hope of life; for, though he died, it was after a very prolonged struggle. This case occurred as far back as the year 1841.

Case VI.—A boy of nine years of age got symptoms of fever, and very soon began to complain of his head, which he could not bear to be stirred. His eyes were much suffused, and the light caused him great distress. Though he had vomited, it was not a prominent symptom. It was clear the brain was seriously threatened; and the case was the more serious as two children in the same family had already died of water on the brain. With this knowledge I did not hesitate to treat the case very actively. Leeches were applied three times within the first thirty hours, and other measures equally active employed. Under these means the head symptoms steadily yielded, and I had the pleasure of being able to pronounce the boy safe by the eleventh day. His ultimate recovery, however, was singularly protracted, and at this period I had the assistance of the late Sir Philip Crampton. This case occurred in 1844, and I think it right to state that in a similar case now I would use a very much modified treatment; for, though the boy recovered, he showed signs of great delicacy for a long time afterwards. He is now however, a vigorous man, close to thirty years of age.

Case VII.—In the year 1849, and at a time when there was a regular run of cases of hydrocephalus, a girl, aged six years, was admitted to hospital with all the signs of the disease, in the second stage, well marked. She had very marked rigidity of the neck, and lay with the head drawn back; the pupils were dilated, one more than the other, and the peculiar cry of the disease was only too plainly heard. The pulse at this stage was slow, and she had several fits affecting one side of the body. I confess I looked on the case as hopeless. What was my surprise to find that, after ten days continuance of these symptoms, signs of amendment gradually, very gradually, began to appear. The intelligence improved, the screaming and rigidity slowly disappeared, and the child finally recovered.

Case VIII.—Hamilton, a boy of six years of age, when first seen in hospital, had every sign of water on the brain. He was very stupid, and had the constant cry of the disease. It is unnecessary to give more particulars, except that he also recovered.

The next case, though ultimately fatal, has too close a connexion with my subject to be omitted here:—

Case IX.—In January, 1855, Robinson, a girl of twelve years of age, was admitted with all the signs of water on the brain. The fever was of a more intense character than is usually present in such cases, but she made constant complaint of her head, whilst her pupils were dilated to the utmost, and the eyes had the look of blindness which all who have seen this disease must have witnessed; there was also vomiting, and she passed under her; the cry, too, was present. Yet from this state the girl recovered. As the brain symptoms, however, lessened, mischief began to declare itself in both the lungs and abdomen; and, in spite of every care, she ultimately sunk at the end of some weeks; I cannot doubt that strumous disease existed in both cavities. This was a remarkable case, and the only one I have seen where the disease ran this particular course. The patient, I should state, had entirely got free from the head symptoms.

Though I could give other cases, I shall limit myself to one more, very recently in hospital, and seen by Dr. Grimshaw and Dr. Barnes, now in Dublin:—

Case X.—Harford, a girl of twelve years of age, was admitted

into the Cork-street Hospital on the 21st of December last. She was tall for her years, and thin, and laboured then under a severe attack of typhus fever, being profusely spotted; the brain was much engaged; she was very restless, and rambled constantly, disturbing the ward by her cries. This state continued longer than usual, but finally subsided, and all the signs of typhus had passed away—so much so, that she asked to be allowed up. At this period she began to complain of pain in her head, referred to the forehead, and with this I found that considerable fever had lighted up; the pulse was quick, the skin hot, and tongue furred. From this on every symptom of water on the brain developed itself. The pulse went through the regular stages of the disease, even to the third; the pupils became dilated; the girl quite stupid, and apparently stone deaf, and the peculiar cry was constant; the arms were rigid, though not in a marked degree, but there was no convulsion. Though she swallowed badly, she did not at any time lose the power entirely. From this state, which lasted about nine days, she gradually emerged, and very interesting it was to observe the daily progress. The dilatation of the pupils was the last symptom to yield; and even when she left hospital, a week since, they gave me the idea of being still sluggish. From the commencement of the brain attack till she left was just five weeks. This was a case of great interest, growing up, if I may so say, under our very eyes, and reaching a stage of the disease from which few indeed recover. Still I trust enough has been advanced this evening to give us better hopes for the future, and it was to add my quota of experience on the matter that I appear before you. The idea of a recovery from hydrocephalus, when it has advanced to all but the last stage, is not mine, though I am not able to refer you to an authority in favour of this view, but it has been so little recognized as to be worthy certainly of farther consideration.

And now, in conclusion, a few words as to treatment. I believe, then, that if the case be seen early, and the nature of the disease, already spoken of, be kept ever in view, treatment holds out a reasonable prospect of success. But to be really useful it must be steadily carried out. By this I mean that one part of the treatment should be made to support another. Once, in fact, a plan is adopted, it should be continuous. I know no disease which requires this rule more than the one under consideration. As surely as the rule is broken—that is, that there are too long intervals between the several measures used, so surely will the disease gain ground,

Keeping this principle then in view, an antiphlogistic treatment should, in the first instance, be adopted. Leeching to the head or nape should very rarely be omitted. The number must, of course, vary with each case, and it has appeared to me better to order a small number twice than the whole number at once. With the leeches purgatives will be also given; and the compound used by Cheyne, consisting of calomel and jalap, is not to be despised. When the proper period arrives, blistering is most useful, but let it be done with a will; for it has happened to me on different occasions to see a blister the size of a crown put on a child's head who had the disease. This is not the way to conquer such an affection, and is only losing precious time. Let the whole head be blistered, and the surface dressed with some irritating ointment, and then good may be done.

For so far I have spoken of measures long known and used; but, I need not add, with anything but satisfactory results, and yet I look upon this part of the treatment as most essential, but it is not to be trusted to alone. And here it is, it appears to me, a decided advance has been made in the treatment; for, while the measures detailed are being put in force, I now give wine at the same time, according to the age of the child. Whether this plan has been adopted by others I cannot say; I do not know of such. It was gradually that it forced itself on my mind, but of its value I have not now the slightest doubt. All the cases given in this paper, which recovered, were so treated, and I may state that the last detailed got ʒvi of wine daily, for a period of twelve days, and this in a child of only twelve years of age. No one, however, will jump to the conclusion that all cases recover because wine is given; this is not what I would convey, nor could it in reason be expected, but only that it holds out the best prospect of success, and has, I firmly believe, been directly instrumental in saving life. It will be observed too that the use of the wine should be begun as early as the case and symptoms allow. In this way, whilst we combat the inflammatory symptoms by antiphlogistics, we, on the other hand, sustain the system by the wine—so that the plan is a union of the two. And still keeping in mind the essence of the disease, it seems to me to have everything to recommend it, in addition to the experience detailed this evening, and to which more could have been added.

Of particular medicines I have little to say. Mercury, so much in use formerly, I now use only as an alterative or occasional

purgative, and seldom, in the former way, beyond the second or third day. I had seen a large number of cases where its specific action was induced, and yet I have notes of but one recovery under these circumstances, and now trust to other means. The medicine which seems to me to answer best is the hydriodate of potash. I had read of its use in these cases, and am sure I have seen benefit from it.[a] Before that time I had myself given other salts of potash, with the idea that their diuretic action would be useful; and one very striking case recovered while the child was getting wine and acetate of potash. To each dose of the hydriodate I also usually add from three to five drops of the tincture of digitalis, as being a medicine which Cheyne considered of value, and used accordingly.

In conclusion, then, I have only again to repeat that the union of the two plans of treatment, the antiphlogistic and the stimulant, are the means on which I now mainly rely in this most intractable disease, and all the cases which I have seen recover have been so treated.

Art. IV.—*On Hospital Dietaries.* By John Beddoe, B.A., M.D., F.S.S.; Physician to the Bristol Royal Infirmary.

The practical application of dietetic science and experience to the victualling of large bodies of men has of late years been frequently a subject of discussion; but such discussion has almost always been set on foot either by some supposed shortcomings in the feeding of soldiers or of paupers, or by real or apparent excess in that of criminals. The experience of our hospitals has seldom, if ever, been called in to throw further light on these matters; nor am I aware that any general view of hospital dietetics has been put forward, except in a short paper by Dr. Steele, late of Guy's, in the Social Science Transactions for 1862. It will be my aim, in the following pages, not so much to insist on the importance of dietetical considerations in therapeutics, as to utilize the practical knowledge of the subject embalmed in the diet-scales of our infirmaries and lunatic

[a] This medicine has, I find, been recommended by several; amongst these are the names of Maunsell and Evanson, Copeland, Willshire, Leney, and Wood of Philadelphia. But none seem to have used it so largely as the late Dr. Coldstream.—(See a paper of his in the Edinburgh Monthly Journal for December, 1859.)

asylums,[a] and to point out a line of investigation whereby it may, perhaps, be turned to better account.

In framing such diet-scales regard must be had to three principal considerations. Of these the advantage of the patients stands, of course, first; economy is the next head; and the third, which is intimately connected therewith, is that of facility of administration.

Let us take the least important first. It has always been a question, about what point the multiplication of regular diets becomes more perplexing and troublesome to the physicians and the commissariat than the necessity of continually superadding "extras." The difference of opinion on this point is well shown by a glance at the respective scales of a few hospitals. Of regular permutations the Bristol Infirmary has four; the Bristol General Hospital has three, or six if we reckon the half diets separately; the Bath United and Worcester are contented with only three; while at Birmingham the medical officers may choose among six, called respectively fever diet, milk, broth, and fish diets, ordinary and extra meat diets. In the scales of certain of the Dublin hospitals, with copies of which I have been favoured by Dr. Kidd, the number of variations never exceeds four; but in the Dublin and Cork workhouse infirmaries, where the abolition of the "extra" system seems to be aimed at, it rises to six, seven, twelve, or upwards, without reckoning those formularies which are meant to apply to children alone.

For myself, I prefer, as more convenient, the principle of the Birmingham scales, though to the manner in which the principle has been carried out I do not mean to subscribe. But it is clear that many physicians are of a different opinion, in favour of which they would allege that though the varieties and idiosyncrasies of peptic ability and need which we meet with in private practice are innumerable, yet almost all hospital patients may most conveniently, though roughly, be classed under three or four schedules.

Of these the first would be formed of patients with acute diseases. These are the proper subjects for *low*, *spoon*, or *fever diet*, which consists usually of little else than bread, which bread forms a sort

[a] I take this opportunity of returning thanks to a number of gentlemen, physicians, secretaries, or governors of divers public institutions, who have kindly furnished me with full particulars of the composition of their diet-rolls; and among these I ought to make especial mention of Dr. R. W. Falconer, who has supplied me with the dietaries of most of the principal prisons in England. I can only regret that Dr. Falconer, having with much trouble made and annotated this collection of statistics, had not sufficient leisure to utilize them to better purpose than I may do.

of *substantia* or material basis whereon may be built up the various accidents of extra diet. Such patients are to be found almost exclusively in the medical wards. Of thirty-five cases on low diet in the Bristol Infirmary only four were surgical. Every one of the thirty-five had some extra or extras allowed him.

Patients with chronic diseases might be ranged, for our present purpose, under three heads. The first (and least numerous in most hospitals) consists of persons afflicted with certain cerebral, hepatic, and cutaneous diseases, often connected with the lithic acid diathesis. For these the *broth* and *milk diets* are generally appropriate. Dyspeptic asthmatics do not come under this head; they generally require a higher quality, but a smaller quantity, of food.

Next comes that large class of chronic cases, often of traumatic origin, in which there is no danger of either plethora or exhaustion, in which we wish neither to starve nor to fatten our patients. This class is particularly numerous in surgical wards, and usually receives what is called the *middle* or *ordinary* diet of the hospital.

The fourth class consists of patients with chronic exhausting diseases, of which phthisis furnishes the best type in the medical, and certain strumous affections, or extensive suppurating wounds in the surgical wards. These require the *full* or highest diet, to which stimulants are often advantageously added.

Convalescents would on this plan hardly require to be classed by themselves: they might often be passed with advantage through all the four diet classes above laid down. If classed at all, they might be divided into those with healthy, and those with disordered digestive and excreting organs. Towards the close of their sojourn in hospital the former should, as a rule, receive the full diet; while the latter, of course, require special and individual consideration. I shall return to the dietetic treatment of convalescents in the latter part of this paper.

Though economy is a consideration very subordinate to that of the patient's benefit, it is one of great importance—one which the officials of hospitals supported by voluntary contributions will find to be demanded of them by the public, even if they themselves do not perceive its moment—one, however, which seems to me to be neglected in certain instances which might be designated. I shall presently point out in figures the great difference between the full (which being, according to Dr. Steele, allowed to two-thirds of the patients, is in effect the ordinary) diet of St. Bartholomew's, London, and the diets most in use in the several country hospitals;

and I think the reader will agree with me that either we country physicians starve our patients, or the St. Bartholomew's men must be rather too liberal to theirs in the costly items of meat and beer. This brings me to the question whether beer should form an integral part of the regular diets in any hospital. I am very decided in the opinion that it ought not to do so, but that it should be ordered as an extra where required. This opinion I base partly on moral grounds, as I am satisfied that a considerable obstacle has been opposed, of late years, to the progress of temperance among the working classes, owing to the spread among them of erroneous or exaggerated ideas as to the favour with which strong drinks are regarded by medical men. And what could be more likely to strengthen these ideas than the fact of beer being allowed, as a matter of course, to every patient not on the lowest diet, as is the case at Worcester and at Bath? At Bartholomew's every patient on the usual diet receives two pints of beer daily, and in several other metropolitan hospitals beer is allowed as a rule, though in University and King's such is not the case.

The absence of beer from the formularies of Scotch and Irish hospitals and lunatic asylums has less weight, as a fact, on the anti-alcoholic side of the question, than it otherwise would have, when taken in connexion with its limited use by the working population of the sister countries; and the same may be said of another fact, which I will nevertheless adduce, viz,, that the dietary framed for the Scottish prisons, by Drs. Christison and Maclagan, under the use of which the standard of health among the convicts has been remarkably high, and the mortality exceedingly low, contains no form of alcohol. The reasons for giving beer a place in the dietary are probably much stronger in the case of lunatic asylums than in that of general hospitals; and it has a place in the tables of all the county and borough asylums in England and Wales (at least of all those whose tables are published by the Lunacy Commissioners) except five. These five are Cumberland, Durham, Cornwall, Hull, and Haverfordwest. In all these, solid and other liquid foods are given in abundance; and if the absence of beer has any bad effect, which I do not suppose, it certainly does not manifest itself in the statistics of deaths and recoveries, which are rather more favourable than the average.

In both the hospitals of Bristol beer is considered as an extra; and that it is not thought generally necessary or desirable by the officers of the Royal Infirmary, is proved by the fact that on a

certain day I found only twenty-one per cent. of the patients in receipt of any form of alcoholic liquor. With respect to economy, alcohol, if it be a food at all, which I myself do not disbelieve, though some have lately denied it, is a very dear form of food, though in some cases it may be a cheap and efficient medicine.

In attempting to compose model dietaries we must pay attention to the tastes, and even to the prejudices, of the probable recipients of the food. The definition of man as a cooking animal hardly includes the lower class of English among the human species. Their range of taste is generally small; and it would be useless to travel beyond its limits, or to attempt to run counter to their prejudices. Thus I find that the Bristol poor always prefer a small portion of meat to a larger portion of soup or other food, though the latter may be both more savoury and more nutritive. Dr. Burder informs me that bread puddings were discarded at the Bristol General Hospital in deference to the repugnance of the patients to what they supposed to be partly composed of scraps; and the very same thing occurred at the Infirmary. There is seldom much craving for variety; but even though that craving be absent, I doubt not that a greater variety from day to day than is allowed in most hospitals would be productive of good, and that more perfect digestion, involving a true economy of nutriment, would accompany the greater gratification of the palate. In most lunatic asylums, and in many prisons, this point is attended to more carefully than in hospitals, being thought more important in these two classes of institutions, on account of the greater duration of the confinement of their inmates, and of their exposition to the influences of a regulated dietary.

I will now proceed to the investigation of the quantity of food, or rather of the quantity of nutritive elements contained in the food, allowed in our diet-scales; its relation to that given in workhouses, lunatic asylums, and prisons, and to that consumed by private individuals; and further, to the consideration of the important question, how much food ought generally to be allotted to the average hospital patient and to the average convalescent.

This part of my subject is beset with numerous difficulties, some of which it is not possible entirely to remove. I have just now spoken of the average hospital patient; but it has been well said that no such thing exists as "the average man," and that no one man is exactly like another, whether in external form or internal constitution. Hospital physicians, of course, recognize and act upon this

fact to a degree which would not be practicable in some other public institutions; nevertheless we are obliged, to some extent, to follow the system of Procrustes; and as he stretched or cut down his patients to the length of his bed, so we must sometimes make ours suit themselves to the diet, rather than suit the diet to them. It is incumbent upon us, therefore, to determine as nearly as possible the value of this imaginary quantity, the average hospital patient considered as a consumer of food.

The greatest difficulties arise from the, as yet, rudimentary state of physiology and animal chemistry, from our imperfect knowledge of the details of the processes of digestion and sanguification, and still more those of destructive metamorphosis. How little do we, as yet, positively know, however much we may conjecture, of the forces which regulate the amount of destructive changes that go on in the human body; of the sources of urea, uric acid, and kreatin, and the circumstances which determine their amount, and which define the quantities of nutriment required to trim the balances of bodily weight, and of muscular and nervous power. In our present ignorance or uncertainty about these things it is difficult, if not impossible, to explain the enormous discrepancies which occur in the consumption of food by different individuals. How was it that Cornaro maintained, as we have read and believe, not only life, but some amount of bodily and mental vigour, for sixty years, on twelve ounces of solid food, and fourteen ounces of wine daily? Or if this may possibly be accounted for by a judicious selection of the most concentrated food, how shall we explain those cases, which almost all of us must have seen, in which animal heat and plumpness, if nothing more, have been kept up, for long periods, on quantities of food which *appear* wholly insufficient, and under circumstances in which, though deception may be suspected, we are convinced that if any nutriment be really smuggled clandestinely into the system, its amount must be but small? Many cases of great and habitual excess in eating, though less wonderful, are also incapable of perfect and satisfactory explanation in the present state of our knowledge; and, at the risk of being set down as a sceptic, I must confess that it is not easy to reconcile them with the current theory on the production of animal heat.

Great difficulties also arise, in forming or comparing diet tables, from the varying composition of sundry kinds of food, and from the fact that some kinds either have never been chemically analysed, or their analyses have been imperfectly executed, or incorrectly

reported. I will mention one or two examples of these difficulties.

Soups and puddings enter more or less conspicuously into most diet-scales, but even where the nature of their several ingredients can be exactly ascertained, the respective quantities can seldom be so, being for the most part unknown to the very concoctors themselves.

Again, the variable proportion of fat in meat causes wide differences in its nutritive value, differences of the greatest importance, but usually much undervalued. The quantity of fat in ordinary meat was by most writers on diet greatly underrated, and by some, wonderful to be said, its presence was wholly ignored, until Lawes and Gilbert showed that meat, especially mutton and pork, fattened to the gross and unnatural degree which is so common now-a-days, ceases to be a pre-eminently nitrogenous food in becoming a highly carbonaceous one. Judging of the proportions by the eye, and from some rough experiments and calculations,[a] I have estimated the cooked meat of the Bristol Infirmary to contain 25 per cent. of pure, dry, lean, or nitrogenous matter, and 20 of fat; and this estimate I have applied to other hospitals; but it is probable that in prisons and workhouses coarser and fatter meat is generally used; and as the meat is often cooked by boiling, 22·5 nitrogenous, and 22·5 carbonaceous would, in those cases, be nearer the mark. In fact, however, the waste of fat varies so much with the mode of cooking and the taste of the consumer, that any estimate of this sort must be often wide of the mark. It is extremely difficult to set down the probable value of broth, beef-tea, &c., even where they are not complicated by the addition of vegetable ingredients, and so converted into soup. The beef-tea of the Bristol Infirmary is rather a gelatinous broth than a true beef-tea, and its proportion of animal to saline matter is, as will be observed in the subsequent tables, rather high.

Even in the article of wheaten bread analyses differ very much.

[a] The carcass of Lawes and Gilbert's "half-fat ox" contained per cent. about 13 of bone, 15 of dry nitrogenized matters in the flesh, 20 of dry fat, and 1 of ash, (in "fat" oxen, and even in store sheep, the proportion of fat was greater.) Assume then that the raw meat of the Infirmary, consisting chiefly of legs of mutton, contains 10 per cent. of bone, 15 of nitrogenous, and 20 of fatty matter. If it loses in roasting 8 per cent. of dripping and 22 of water, there will remain 60 of meat, containing 15 nit. and 12 fat, or per cent. 25 and 20. The loss of dripping is often greater; in boiling, on the other hand, less fat is lost to the meat, but a notable proportion of nitrogen goes into the broth.

Christison, in his earlier calculations at least, assigned to it 10½ per cent. of nitrogenous matter; but Maclagan found an average of less than 8 per cent. in Edinburgh bread. I think these differences depend mainly on the quality and country of growth of the wheat; Lawes and Gilbert, operating on bread made from English flour, found a little more than 8 per cent. Dr. Griffin, of this city, analysed for me two samples of good seconds flour employed in the Bristol Infirmary, and found therein an average of nearly 1·99 per cent. of nitrogen, which indicated, according to my calculations, the presence of fully 9 per cent. of plastic matters in the Infirmary bread. I am informed that the difference of texture in bread and flour renders it somewhat more difficult to ensure perfect combustion in the case of the former; and this may possibly account for some analyses, in which dry bread appears to contain less nitrogen than the flour of which it is made taken in the same dry condition. I have thought it safest to assign to bread 8·5 per cent. of nitrogenized constitutents.

Arithmetical blunders, and want of practical acquaintance with the subject, have resulted in the publication of some curiously erroneous analyses. Milk of all sorts has been examined many a time, but nobody seems to have thought it worth his while to make an analysis of cream, until Professor Völcker, of Cirencester, supplied the void. The illustrious Berzelius had once published a partial one of an ordinary specimen of *milk*. I do not know whether the great chemist's pen had slipped, or whether his English translator had a bad dictionary, but so it fell out that the analysis was published in England as referring to *cream*. Pereira copied it into his work on diet; and from that time to this almost every writer on the subject has done the same, down to Dr. Dobell, who published a useful analytic table a few months ago. One *savant* only, who had probably been outside of London, and seen that the cream was at the top of the milk, seems to have been struck with the singularity of a cream containing only 4½ per cent. of butter, and he qualified Berzelius' fluid as "a specimen of *thin* Swedish cream!" I myself found, in an ordinary sample, 27 per cent of butter, or six times as much as was set down by these authors, none of whom, I suppose, had ever heard of the rough farmer's rule, "a quart of cream to a pound of butter." Dr. Völcker found 25·8 per cent.

For the sake of perspicuity, in the face of all these doubts, mistakes, and difficulties, I have thought it necessary to prefix to my

chemical analysis of dietaries a standard table of the composition of food. It is on a similar plan to that of Dr. Dobell; but it will be observed that in some of its items it does not exactly correspond with his or any other such table. In framing it I have consulted a great number of authorities, British and Foreign, not forgetting, of course, to delve in the mine of Moleschott. Where I found great diversity of statements I have not contented myself with a simple average of the whole, but have often given more weight to the dicta of individual analysts who seemed to deserve it.

The last difficulty I shall dwell upon is, that practical proof of the *perfection* of a diet-scale is unattainable. It is not enough to say "so many people, such and such bodies of men, have lived in this way for a length of time and found it answer very well." It may be so; but a different regimen, if tried, might have answered still better: who can say? Whole nations go on, year after year, and generation after generation, committing gross dietetic errors without finding them out. I suppose the Westmann islanders would never themselves have discovered that their diet of sea-fowls' eggs and stinking fish had anything to do with the death of all their infants from trismus. The Färoers are said to prefer their food putrid; yet most of them live on it to a good old age, though it entails on them habitual diarrhea. Norfolk dumplings are accused of producing the stones in which that county is so fertile; and Balassa, the eminent Hungarian surgeon, speaking to me of the frequency of both cataract and calculus among the peasantry of his country, was disposed to ascribe it to their diet of boiled flour and grease. In the northern parts of this island, at the present day, the mass of the population is gradually, voluntarily, and without any other motive than taste or fashion, deserting a better for a worse, a stronger for a weaker diet; and the substitution of white bread and tea for oatmeal and milk will probably soon manifest its effects in a notable deterioration of the breed.

Standard Table of Composition of Food.

—	Nitrogenous	Fatty	Amylaceous	Indig. or Complem.	Mineral	Water
Bread,	8·5	1·	52·	1·	1·5	36·
Wheat flour,	12·	1·25	72·	1·	1·	12·75
Oatmeal,	16·5	6·	60·	2·5	2·5	12·5
Maize-meal,	11·	7·	65·	3·	1·	13·
Rice,	6·5	·5	80·	1·	·5	11·5
Pearl barley,	9·	1·	75·	1·5	1·	12·5
Peas,	22·5	2·	50·	9·	2·5	14·
Potato,	1·75	?	17·5	4·5	1·25	75·
Do., cooked and skinned,	2·	?	20·	2·75?	1·25	74·
Carrot,	1·5	·2	8·5	3·5	1·3	85·
Other vegetables, average estimate,	1·	—	5·	—	—	—
Meat, cooked, boneless (hospitals),	25·	20·	—	—	2·	53·
Do., do., (prisons),	22·5	22·5	—	—	2·	53·
Do., with bone,	20·	20·	—	13·		47·
Do., raw, bony,	15·	20·	—	10·		—
Do., raw, boneless,	16·6	22·5	—	—	1·	60·
Bacon,	8·	75·80	—	—	?	?
Bones (varies with mode of cooking),	—	—	—	—	—	—
Egg (calculated on two ounces),	12·5	10·	—	—	1·	76·5
New milk,	4·	3·5	4·5	—	·5	87·5
Skimmed milk,	4·	·5	4·5	—	·5	90·5
Cheese,	30·	27·	3·	—	5·	35·
Butter,	2·5	87·5	—	—	2·	8·
Suet,	1·	95·	—	—	—	4·
Sugar, common raw,	1·	—	92·	—	1·	6·
Cocoa,	10·	50·	20·	—	?	?
Beer, average,	·05	—	6·	Alcohol	?	?
Beef-tea,	3·	—	—	—	1·	96·

—	WEEKLY																			DAILY				—
	Bread, oz.	Flour, oz.	Oatmeal, oz.	Maize-meal, oz.	Rice, oz.	Barley, oz.	Peas, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c., oz.	Sugar or Molasses, oz.	Cocoa, oz.	Milk, oz.	Beer, pints.	Extras	Nitrogenous, oz.	Fatty, oz.	Amylo-saccharine, oz.	Total	Informant
Bristol Royal Infirmary (full),	84	–	–	–	–	–	–	42	–	–	42	–	–	–	–	70	–	—	3·04	1·67	7·90	12·61	—
Do. (middle),	84	–	–	–	7	–	–	21	–	–	21	–	–	1·5	–	140	–	—	2·70	1·42	8·74	12·86	—
Do. (soup),	70	–	–	–	21	–	–	–	7	–	–	–	–	1·5	–	140	–	Soup, bones	2·02	1·03	9·10	12·15	—
Bristol Gen. Hospital (extra),	126	–	7	–	–	–	–	42	–	–	42	–	–	–	–	70	–	—	3·71	1·79	11·61	17·11	Dr. Burder
Do. (ordinary),	84	–	7	–	–	–	–	28	–	–	28	–	–	–	–	70	–	—	2·66	1·33	8·09	12·08	Do.
Do. (milk),	84	–	7	–	–	–	–	–	–	–	–	–	–	?	–	70	–	Soup and pudding	2·68	1·33	8·90	12·91	Do.
Worcester (house),	98	–	–	–	–	–	–	48	–	–	18	–	–	3·	Tea	16	7	Bth. soup 1 pint	2·20	·80	10·82	13·82	Dr. Inglis
Do. (full),	98	–	–	–	–	–	–	48	–	–	24	–	–	3·	Tea	16	7	Bth. soup 1 pint	2·40	1·00	10·82	14·22	Do.
Bath United (half),	84	–	–	–	–	–	–	32	–	–	16	–	3·5	10·?	Tea	80 ?	7	Pudding 3 days	2·59	1·58	11·23	15·40	Mr. Roberts
Do. (full),	84	–	–	–	–	–	–	48	–	–	24	–	3·5	7·	Tea	64	7	Pudding 1 day	2·55	1·60	10·70	14·85	Do.
Bath Min. Water Hosp. (ord.),	112	–	–	–	4	–	–	44	–	–	32	–	7·6	4·5	Tea	17	4	Soup 3 days	3·07	2·29	11·92	17·28	Mr. Starr
Taunton (house),	112	–	–	–	–	–	–	56	–	–	28	–	3·	–	Tea	35 ?	–	—	2·77	1·5	10·82	15·09	Rev. W. Jones
Do. (full),	112	–	–	–	–	–	–	56	–	–	42	–	3·	–	Tea	35 ?	–	—	3·27	1·9	10·82	16·	Do.

—	WEEKLY																		DAILY				—
	Bread, oz.	Flour, oz.	Oatmeal, oz.	Maize-meal, oz.	Rice, oz.	Barley, oz.	Peas, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c., oz.	Sugar or molasses, oz.	Cocoa, oz.	Milk, oz.	Beer, pints	Extras	Nitrogenous, oz.	Fatty, oz.	Amylo-saccharine, oz.	Total oz.	Informant
Birming., Queen's Hosp. (ord.),	84	–	7	–	–	–	–	56	–	42	–	–	–	3½	Tea	79	–	–	2·64	1·25	9·42	13·31	Dr. Fleming
Do., do., (extra),	84	–	7	–	–	–	–	56	–	84	–	–	–	3½	Tea	79	14	–	3·27	1·93	11·82	17·02	Do.
Bartholomew's, (full),	98	–	–	–	–	–	–	56	–	– Bones	56	–	7	5¼?	Tea	–	14	–	3·52	2·71	12·08	18·31	—
Glasgow, (ord.),	98	–	–	–	–	18	–	–	Vary	14	28	–	7	8¾	Tea	12	–	–	2·79	2·20	11·	16·	Mr. Thomson
Do., (milk),	56	–	–	–	17½	–	–	–	–	–	–	–	–	–	–	420	–	–	3·24	2·19	8·86	14·29	Do.
Edinburgh, (common),	84	–	–	–	–	7	–	112	5¼	14	–	–	–	–	Tea Coff.	21	–	–	1·92	·62	11·18	13·72	Mr. M'Dougall
Do., (steak),	84	–	–	–	–	7	–	112	5¼	42	–	–	–	7	Tea Coff.	21	–	–	2·62	1·12	11·18	14·92	Do.
Do., (full),	21	–	31½	–	–	7	–	224	5¼	56	–	–	–	–	140 B.milk	70	–	–	4·19	1·82	12·71	18·72	Do.
Dublin, Richmond, Whitworth, Hardwicke, (mid. with fast),	112	–	3½	–	–	2½	–	–	2½	20	–	–	–	7¾	Tea	17½	–	–	2·07	1·15	10·08	13·30	Mr. M'Mullen, per Dr. Kidd
Dublin, Meath, (mid. with fast),	112	–	27	–	–	–	–	–	Herbs	–	–	–	–	3½	Tea	146	–	Broth	3·	1·25	12·56	16·79	Mr. Anderson, per Dr. Kidd
Dublin, R.W.H., (mutton),	112	–	–	–	–	–	–	–	–	56	–	–	–	7	Tea	17½	–	–	2·80	1·24	9·36	13·40	—
Do., (full with fast),	116	–	4¼	–	–	5	–	48	5	40	–	–	–	7¾	Tea	17½	–	–	2·75	1·58	11·87	16·20	—
Dublin, Meath, (full with fast),	112	–	27	–	–	–	–	–	Herbs	40	–	–	–	3½	Tea	146	–	–	3·80	2·25	12·56	18·61	—

—	WEEKLY																		DAILY				—
	Bread, oz.	Flour, oz.	Oatmeal, oz.	Maize-meal, oz.	Rice, oz.	Barley, oz.	Peas, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c., oz.	Sugar, Molasses, &c., oz.	Cocoa, &c., oz.	Milk, oz.	Beer, pints	Extras	Nitrogenous, oz.	Fatty, oz.	Amylo-saccharine, oz.	Total	Informant
North Dublin Union, Convalescents (No. 2, 2, mutton, one of best), .	132	–	–	–	–	–	–	–	–	–	36	–	–	5¼	–	157½	–	–	3·66	2·13	12·93	18 72	Dr. Minchin, per Dr. Kidd
Do. (Appendix No. 3–5, best of infirm and epileptic ds., .	84	–	31½	10½	–	–	–	–	–	–	–	–	–	–	–	210	–	–	3·13	1·52	11·26	15·91	—

In those hospitals in which tea, sugar, and butter are not given, the patients are usually allowed to introduce them themselves; this is the case at Bristol, where the amounts in the second and third columns therefore appear rather too low.

LUNATIC ASYLUMS.

Sussex County, . . .	126	5	–	–	–	–	–	32	36	36	–	–	3½	11½	Coff. 3½	56	3½	–	2·85	2·86	13 64	19·35	—
Do., workers, extra, .	12	–	–	–	–	–	–	–	–	–	–	6	–	–	–	–	6	–	3·26	3·11	15·58	21·95	—
Leicestershire and Rutland, .	108	6	¾	–	–	–	¾	66	7	24	2	5¼	6½	Tea Coff.	43½	3	–	2·94	2·10	11·83	16·87	Mr. Buck
Do., workers, extra, .	24	–	–	–	–	–	–	–	–	–	6	–	–	–	–	6	–	3·50	2·37	14·52	20·39	—
Wilts County, . . .	98	9¾	–	–	3¼	–	2½	65	–	16½	14	5	5⅓	Coff.	17½	9⅓	Soup	3·01	2·11	12·70	17·82	Dr. Thurnam
Cumberland, . . .	78	12	42	–	–	1½	–	50	–	12?	12½	2	5½	1½	–	312	–	–	4·91	3·86	14·43	23·20	—

—	WEEKLY																		DAILY				—
	Bread, oz.	Flour, oz.	Oatmeal, oz.	Maize-meal, oz.	Rice, oz.	Barley, oz.	Peas, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c., oz.	Sugar, Molas., &c., oz.	Cocoa, oz.	Milk, oz.	Beer, pints	Extras	Nitrogenous, oz.	Fatty, oz.	Amylo-saccharine, oz.	Total	Informant
Armagh District Asylum, .	64	–	63	–	–	3·5	–	48	63	32·6	–	–	–	–	–	225	–	–	4·55	2·71	13·74	21·00	Dr. M'Kinstry
Do., workers extra,	48	–	–	–	–	–	–	–	–	–	–	–	–	–	Tea	–	–	–	5·15	2·78	17·38	25·31	—
Sligo and Leitrim, . .	82	4	40½	28	6	–	–	50?	–	26	–	–	–	1	–	160	–	Bones 18	4·31	2·65	16·46	23·42	Mr. M'Munn
Abergavenny, . . .	114	–	–	–	–	–	1 qrs	100		·31	–	5	5·5	8	–	20	or cider 5¼	–	2·73	2·29	12·80	17·82	Dr. M'Cullough
Do., workers extra,	12	–	–	–	–	–	–	–		–	–	6	–	–	–	–	6	–	3·14	2·54	14·74	20·42	—

In almost all lunatic asylums there is an extra allowance for workers; but I have not given this in every case.

WORKHOUSES.

Clifton Workhouse, . .	113	9	13½	–	4	–	6	48	–	–	9	10⅓	2	–	–	52½	–	–	3·29	1·61	13·16	18·06	—
Bath Workhouse, . .	132	–	12	–	–	–	3	24	–	–	13	18⅓	–	–	–	–	–	–	3·22	1·82	11·81	16·85	—
Cork Union, . . .	112	–	10½	56	4	–	–	–	21?	–	–	–	–	–	–	105	–	–	3·24	1·	14·73	18·97	—

PRISONS.

—	WEEKLY																		DAILY				—
	Bread, oz.	Flour, oz.	Oatmeal, oz.	Maize-meal, oz.	Rice, oz.	Barley, oz.	Peas, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c., oz.	Sugar, Molas., &c., oz.	Cocoa, oz.	Milk, oz.	Beer, pints	Extras	Nitrogenous, oz.	Fatty, oz.	Amylo-saccharine, oz.	Total	Informant
Scotch prisons, first rate,	30	–	54	–	–	15	–	176	?	5	–	–	–	–	–	Skim 165	–	–	3·29	·83	13·48	17·60	Prof. Christison
Do., second rate,	48	–	68	–	–	20	–	176	?	7	–	–	–	–	–	Skim 165	–	–	3·94	1·05	16·61	21·60	Do.
Do., third rate,	76	–	74	–	–	20	–	208	?	7	–	–	–	–	–	Skim 165	–	–	4·50	1·14	19·88	25·52	Do.
Dorset County, medium rate,	172	–	25	–	–	4	–	32	2	–	12	6	–	-	–	–	–	–	3·60	·88	16·53	21·01	Dr. Falconer
Bristol prisons (medium rate, class 4, 7, 8),	168	–	28	–	–	3	–	41	–	–	9	–	–	–	–	–	–	–	3·14	·77	16·37	20·28	—
Morpeth prison (medium rate, class 4),	48	9	84	–	–	3	–	33	3	12	9	–	1	–	–	70	–	Bones 12	3·82	2·01	13·43	19·26	Mr. Birch, Gov.
Usk Gaol (exceeding four weeks),	56	–	98	56	–	–	–	–	–	–	–	–	–	–	–	210	–	–	5·07	2·53	19·01	26·61	Dr. Falconer
Do., improved,	68	–	98	24	–	–	–	24	–	–	8	–	–	–	–	170	–	Broth	4·85	2·67	17·41	24·93	Do.
Convicts on public works, light labour,	145	8	14	–	–	2	–	96	7½	24	–	4	1½	5	3½	14	–	–	3·26	1·47	14·65	19·28	—

—	Nit.	Fat	Amy.	Total
Six male Lancashire operatives, during "cotton famine," calculated from E. Smith's data, . . .	3·17	1·94	16·54	1·55
A very poor Northampton shoemaker, also from E. Smith's data, ...	2·27	2·05	11·39	5·71
A very poor Staffordshire labourer, do., do., two-fifths of consumption of man, wife, and two small children, .	2·18	1·62	12·68	16·48
A Gloucestershire labourer, do., do., one-half consumption of man and wife, ...	4·27	2·43	25·	31·70
Military hospitals, low diet, ...	2·65	2·50	11·15	16·30
Do., half diet, ...	3·34	2·	14·14	19·48
Do., extra diet, ...	4·17	2·80	15·81	22·78
Leicestershire Asylum, male attendants' diet (strictly adhered to, but alcohol in addition), ...	3·96	3·42	14·52	21·90

Perhaps the most obvious conclusion to be drawn from the foregoing tables is that the prison diets are generally more liberal in quantity than those of the lunatic asylums and workhouses, while those of the hospitals, as a rule, stand last. It may be that the prison diets, examined from a moral and economical, as well as a physical point of view, may be generally too high, or at least too large in quantity; but so far as their effects on the health of the convicts are concerned, there is probably very little fault to be found with them. In the Scotch prisons, for example, the rate of mortality is low, and the standard of health high; and in certain experiments made in Morpeth Gaol (the most usual dietary of which I have selected for examination) it was found that convicts exactly maintained their initial weight. Allowing, however, for the partial failure of digestive power which is reported to be frequent among prisoners, it is probable that these prison dietaries are more copious than would be necessary for other persons undergoing similar amounts of exertion. There are great differences as respects quantity among the lunatic asylums—so great that one is led to suppose, at first sight, as in the cases of certain hospitals before mentioned, that there must be deficiency or excess in one or both of the extremes. The investigation of the matter is a little complicated by the presence of beer in the ordinary diet scales of most of the English asylums, including all those where deficiency in other respects might be

suspected. The scale is remarkably low at Abergavenny, where it works very well, and where, Dr. M'Cullough informs me, there can be no question that the patients are well nourished, and in good condition; and it is not much higher at Leicester and Devizes, where Mr. Buck and Dr. Thurnam find the quantities sufficient. The latter, indeed, reports to me that the Wiltshire labourers are apt to get fat on their asylum allowances. In the Irish asylum diets the quantities appear rather large; but they are exceeded in amount of carbon by some English ones.

Dr. Edward Smith's official reports to the Privy Council might be expected to afford much valuable information as to the minimum amount of food necessary for the sustention of health; but only a small portion of this large collection of facts is available for my present purpose. His reports of the dietary of independent labourers are necessarily almost always complicated by the presence of women and children in the families; moreover, the ideas of the labouring class, generally, in England, as to their expenditure for food, are so very hazy, that we cannot but regard the reports with great suspicion. It is, in fact, a matter of extreme difficulty to ascertain what quantities of food are ordinarily consumed by that class of persons which furnishes the inmates of hospitals; but there is no doubt that if most of them do not eat much more when at home in health than when in hospital, "it is their poverty, and not their will" that consents to this temperance. I have computed the nutriment in two or three of Smith's cases, where a very varied amount of nourishment seems to have concurred with the maintenance of health. But I have much more confidence in his account of the diet of six male Lancashire operatives, during the recent depression of the cotton trade, which also I have computed according to the system adopted in this paper. It ranged much higher than our infirmary, or even our workhouse diets; but the excess was chiefly or altogether in amylaceous food. This observation, which appears to have been carefully made, has the more value as we now know that the population of Lancashire, generally speaking, was sufficiently well fed during the "cotton famine" to maintain a fair standard of health, though the quantity and quality of their food were both below what was customary with them. I have myself ascertained the consumption of a family of six adults, not stinted by lack of means, to be about 3·8 oz. nitrogenous, 2·9 fatty, and 12·5 amylaceous, per head, per diem, which about equals the full diet of St. Bartholomew's, but considerably exceeds most hospital

diets. The average amount of nitrogenous food obtained by outdoor labourers and their families in England, is, if we accept Edward Smith's reports, about 3·5 per diem, per computed adult. But while on the one hand, persons of sufficient income and active habits generally take a very much greater quantity of nitrogenous food without any apparent detriment, the body when in perfect rest certainly requires very much less. The experiments of Gasparin and of John Ranke (Parkes on Hygiene, p. 138), certainly yield results, as to nitrogen, too high to be generally applicable. Ranke required 240 grains nitrogen (= 3·47 oz. nitrogenous matter), and 3,531 grains carbon (= 8·04 oz.), to exactly maintain his weight, and the balance between ingesta and egesta. But in the first place these figures should be considerably reduced to render them applicable to ordinary men, as this physiologist has a remarkably tall and fine frame; and in the second, it seems probable that when a man accustomed to live plentifully, submits to a disagreeable degree of abstinence, he "frets his tissues" to a degree that may vitiate the experiment.

The question may now be put—are the quantities allowed in our hospital diet-scales sufficient? There are several undeniable reasons why they should be smaller than in most of the dietaries with which I have compared them. Of these, the comparative want of exercise, and consequent lessened waste of the tissues, is a chief one, to which we may add in winter the lessened exposure to cold. Again, we have the power of supplementing a diet which appears relatively deficient by extras;[a] and there may therefore be much less risk in a deficient than in an excessive standard. Thirdly, it is of course wrong to judge of the nutritive quality of food exclusively by its chemical composition; and the food of an infirmary, of better quality and better cooked than what working men commonly obtain, may prove more nutritive, because more tempting and digestible, than a large quantity of coarse or insipid aliment. It is curious that the most liberal dietary (chemically speaking), of those which I have analyzed, is that of Usk gaol, consisting chiefly of Indian meal and oatmeal, with milk; but though it would probably have sufficed to maintain the vigour of a hardy Scotch peasant

[a] In military hospitals it is the endeavour to do away, as far as possible, with the system of extras (Parkes on Hygiene); in accordance with this aim, it will have been observed, that the dietaries there are much higher than in civil hospitals. Another motive for this liberality in scale is the desire to fit the convalescents for duty as soon as possible.

under much exertion and exposure, I am informed by Dr. Falconer that it was not found to suit the health of prisoners long in confinement; and, I believe it was not its excessive quantity but its insipidity and, potentially viewed, its scantiness, which was thought to be in fault. It contained no meat, but the quantity of proteineous nutriment was far greater than in an ordinary meat diet; and when it was improved in favour of certain classes of prisoners, by the substitution of a portion of meat and potatoes for meal and milk, it was, according to chemical notation, considerably reduced.

It is now some time since I conceived the idea of testing the sufficiency of the diets of the Bristol Royal Infirmary, by weighing the convalescents and chronic invalids at intervals, and at the same time examining the urine, roughly determining the amount of urinary solids, and thus conjecturing, if not absolutely determining, the extent of waste or deficiency of food. The observations of the urine, and of the weights, have not been conducted with so much scientific accuracy as would have been practicable in clinical wards; but, I trust they are of sufficient interest and value to deserve publication.

Experiments on Convalescent Diet in Bristol Infirmary.[a]

M'Carthy, forty-nine, fish-hawker, formerly soldier, 5 feet 9 inches; weight, when in good health, 160 lbs.; phthisis in first stage, with marked symptoms; improving. Taking iron and quinine; no butter, but ʒvi. of cod-oil daily. January 30th. Weight 148 lbs., diet middle—bread 4 oz., milk ½ pint, beef-tea ½ pint, cod-oil ʒvi., sugar 1 oz., or 3·77, 2·55, 12·27=18·60. February 9th. Weight, 150 lbs. 13th. Urine of 2 days' average, 49 oz., sp. gr. 1017, solids 833 grs., urea (by Haughton[b]) 392. 17th. Weight 152½ lbs., diet full—milk 1 pint, beef-tea ½ pint, cod-oil ʒvi., sugar 1 oz. =4·17, 3·11, 9.80=17·08. 23rd. (Very cold weather.) Urine of 2 days' average, 106 oz., sp. gr. 1010, solids 1060, urea (Haughton) 544. 24th. Weight 154 lbs. March 1st. Urine of 2 days' average, 66 oz., sp. gr. 1012, solids 792, urea (Haughton) 390, weight

[a] I have made such allowances for butter and sugar as appeared probable. The words *full, middle, soup,* refer to the diet scales of the Infirmary. The men were always weighed at the same period of the day, and in the same clothes, if possible. No urine was lost at stool; and no patient suffering from diarrhea was included.

[b] I have estimated the urea by Haughton's plan, though I could wish that he would give us more certain ground for relying on it; U (H) signifies urea by Haughton's rule. The greater the quantity of food the greater is that of urophane salts; hence in the convalescents the urea is probably put too high.

155 lbs. March 4th. Weight 155 lbs., diet full—bread 4 oz., egg 1, cod-oil ʒvi., sugar 1 oz.=3·66, 2·64, 11=17·30. 9th. 2 days' urine, average 66 oz., sp. gr. (?). 11th. Weight 156 lbs. (?). 16th. 1 day's urine, 48 oz., 1015, solids 720, urea (Haughton) 340. 18th. Weight 155¼ lbs., diet, full—bread 4 oz., egg 1, milk 1 pint, cod-oil ʒvi., sugar 1 oz.=4·46, 3·34, 11·88=19·68. 23rd. 1 day's urine, 66 oz., 1016½, solids 990, Urea (H) 467. 24th. Weight 156½ lbs.

This man had ceased to improve in weight and appearance on the third diet, but when a pint of milk had been added to it began to mend again in both respects, though the urinary solids appear to have increased with the nitrogen in the diet.

Reynolds, twenty-five, shoemaker, 6 feet, healthy weight unknown, convalescent from enteric fever; quinine. December 24th. Weight 132½ lbs., diet, middle—bread 6 oz., beef-tea 1 pint, milk ½ pint, butter 1 oz., sugar 1 oz.=4·24, 2·85, 13·30=20·39. 28th. Urine, 2 days' average, 72 oz., sp. gr. 1009, solids 648 grs., urea (Haughton) 360 (?). 30th. Weight 138 lbs., increase of 5½ lbs. in 6 days. A large diet, but evidently no surplusage; the urinary solids being very low, and the increase great.

Powell, about sixty, healthy weight unknown; had had an ulcer occupying the whole visible portion of the pharynx, which almost wholly prevented him from swallowing even liquids, so that he depended on sucking underdone meat; he had been much emaciated, but the ulcer was almost healed when the observation was begun. Diet full—milk-bread 8 oz., 2 eggs, milk 1 pint, rice pudding 8 oz., beer 1 pint, sherry 2 oz., butter 1 oz., sugar 1 oz.= 5·80, 4·20, 15·65=25·65. Increase in weight, 13½ lbs. in 8 days; urine unfortunately not examined.

This is the most rapid increase I have ever observed, and seems hardly explicable even by the very copious diet. I do not think he got any food without my knowledge. It is possible that an unusual proportion of the increase consisted of water, as he had for many days been unable to take any fluid.

Ch. Williams, nineteen, labourer, recovering from typhoid fever, complicated with pneumonia and pleurisy. January 23rd. Weight 118 lbs., diet middle—extra bread 6 oz., egg 1, beef-tea 1 pint, milk 1 pint, beer 1 pint, cod-oil ʒvi., butter and sugar=4·90, 4·, 14·90=23·80. 29th. Urine 82 oz., sp. gr. 1016, solids 1312, U(H) 634. 30th. Weight 119½ lbs; diet on 30th only 3·64, 3·34, 11·78=18·76, urine 60, 1012, solids 720, U(H) 355. 31st.

Urine 60, 1015, solids 900, U(H) 426. February 1st. Urine 48, 1020½, solids 984, U(H) 490. The urine on and after this day was collected and estimated as follows: the urine attributed to each day was taken from the vessel at the close of the evening of that day. February 2nd. Urine 66, 1013, solids 858, U(H) 431, diet 3·64, 3·34, 11·78=18·76. 3rd. Urine 66, 1015, solids 990, U(H) 468, diet 6·06, 4·66, 18·=28·72. 4th. Urine 72, 1014, solids 1008, U(H) 489, diet 4·90, 4·, 14·90=23·80. 5th. Urine 62, 1012, solids 744, U(H) 366, diet 3·64, 3·34, 11·78=18·76. 6th. Weight 125 lbs.; gain 5½ lbs., or 8 lbs. per diem, urine 60, 1014, solids 840, U(H) 408, diet 4·90, 4·, 14·90=23·80. 7th. Urine 62, 1010, solids 620, U(H) 319, diet 6·06, 4·66, 18·=28·72. 8th. Urine 62, 1011, solids 682, U(H) 329, diet 4·90, 4·, 14 90= 23·80. Same diet continued. The previous changes had been effected by sometimes withholding and sometimes doubling the 6 oz. extra bread, and the 3 oz. cooked meat. 13th. Weight 129½ lbs.; gain, 10 lbs. in a fortnight on the preceding diet; diet full—extra bread 6 oz., 1 egg, ½ pint beef-tea, ½ pint milk, ½ pint beer (the reduction of beer did not take effect till the 16th), butter and sugar, cod-oil ʒvi., or 4·54, 3·90, 12·97=21·41. 14th. Urine 70, 1013, solids 921, U(H) 456. 15th. Urine 78, 1015, solids 1170, U(H) 553. 16th. Urine 68, 1017, solids 1156, U(H) 544. 17th. Urine 84, 1015, solids 1260, U(H) 596. 18th. Urine 74, 1014, solids 1036, U(H) 503. 19th. Urine 72, 1019, solids 1368, U(H) 838, diet reduced by abstraction of 6 oz. bread, 4·03, 3·84, 9·85= 17·72. 20th. Weight 134½ lbs.; gain 5 lbs., diet as yesterday continued, urine 76, 1017, solids 1292, U(H) 608. 21st. Urine 60, 1022, solids 1320, U(H) 772. 22nd. Urine 68, 1021, solids 1428, U(H) 847. 23rd. 62, 1020, solids 1240, U(H) 746. 24th. Urine 72, 1015, solids 1080 U(H) 511. 25th Urine 76, 1014, solids 1064, U(H) 516, diet, 2·53, 2·64, 985=15·02. 26th. Urine 77, 1015, solids 1155, U(H) 546, diet 5·53, 5·04, 9·85=20·42. 27th. Urine 53, 1019½, solids 1034, U(H) 627, diet 4·03, 3·84, 9·85=17 72; weight 137 lbs.; gain 2½ lbs. 28th. Urine 42, 1020 solids 840, U(H) 506, diet 4·03, 3·84, 9·85=17·72. 29th. Urine 52, 1023, solids 1196, U(H) 692. March 1st. Urine 46, 1021, solids 966 U(H) 573. 2nd. Urine 48, 1021, solids 1008, U(H) 598. 5th. Weight 146 lbs.; gain in 7 days, 9 lbs., in 14 days, 11½ lbs. On the 25th the meat had been withheld, and on the 26th doubled.

I think I am entitled to draw some inferences from this in-

teresting case. During the whole period the patient's pulse was quick and feeble, but gradually becoming less so; he was also inclined to perspire freely, especially in the early part of the experiment, when he was wholly confined to bed. There were suspicions of tuberculosis. I think the comparatively low amounts of solids in the first half of February may have been partly due to excretion of urea by sweating. It would seem that the amount of amylo-saccharine food was larger than necessary at first, for its reduction had no apparent effect on the rate of increase. The effect of the occasional variations in the quantity of meat and bread was probably beneficial; the doubling of the meat had very little influence on the solids of the urine, whether on the same or the succeeding day. I reduced the bread on the 19th February because the patient had for some days appeared "heated" and oppressed, while the urinary solids increased. The reduction had, however, no immediate effect, and perhaps the feverish symptoms were due to some other cause than excess of food.

Maher, fifty-four, labourer, 5 feet 9 inches; chronic nasal catarrh; improving; doubtful syphilitic taint; taking small doses of iod.-ferri and quinine. December 7th. Weight 151½ lbs., diet middle—extra bread 4 oz., beef-tea ½ pint, sugar, little butter= 3·37, 1·75 (?), 11·82=16·94, urine 2 day's average 55 oz., sp. gr. 1016, solids 880, urea (Haughton) 415. 15th. Weight 151½ lbs. (unaltered), diet middle—bread 6 oz., beef-tea ½ pint, milk ½ pint, sugar and butter as above=3·94, 2·12, 13·31=19 37, urine 2 days' average, 72 oz., 1014, solids 868, urea (H) 421. 21st. Weight 153½; gain 2 lbs. In Maher's case the first diet was probably insufficient; for its increase fattened the man without affecting the urinary solids.

Hill, twenty, brickmaker, of rather large frame; slight rheumatism. February 23rd. Weight 153½ lbs., diet soup, with butter and sugar= 2·05, 1·85, 10=13·90; in bed. 27th. Weight 151 lbs., diet middle—with b. and s.=2·72, 2·24, 9·64=14·58. March 5th. Weight 151 lbs., urine 72, sp. gr. 1014, solids 1008, urea (Haughton) 489. 9th. Weight 150 lbs., diet full—4 oz. extra bread, b. and s.=3·41, 2·54, 10·88=16·83. 12th. Urine 82, 1011, solids 892, urea (Haughton) 434. 17th. Weight 153 lbs.

The first diet was evidently quite insufficient for this man, even while in bed; though he was not suffering much, or otherwise showing signs of requiring much food. The second was not quite

sufficient; the third was rather more than was necessary, but not more than was desirable.

Lewis, about forty, very small man; recovering from severe bronchitis. February 18th. Weight 107½ lbs., diet middle, ½ pint beef-tea, 2 oz. brandy, b. and s.=3·02, 2·25, 9·64=14·91. 27th. Weight 108½, ʒ ss., cod-oil added, and 1 oz. brandy taken away, 3·02, 2·75, 9·64=15·41. March 9th. Weight 115 lbs.; gain 6½ lbs. in 11 days.

Sheppard, thirty-six, excavator, 5 feet 2 inches, average weight about 120 lbs.; recovering from sciatica; an epileptic. Treatment, warm baths, hypo-phosphite of soda. No fits while under treatment. February 18th. Weight 117 lbs., diet middle—b. and s.= 2·73, 2·25, 9·64=14·62. March 6th. Weight 119¼ lbs. Here the diet was, probably, exactly right.

Burris, thirty-five, forgeman, height 5 feet 4 inches; strong muscular man, with some remaining hemiplegia from an apoplectic attack two years before. March 25th. Weight 152 lbs., diet middle—with 1 oz. sugar, no tea=2·71, 1·72, 9·65=14·08; 12 grains iod. pot. daily. 30th. Urine of 4 days' average, 37 oz., sp. gr. 1025, solids 925 grains, urea (H) 510 April 3rd. Weight 152¼ lbs., 6 grains iod. pot., 30 grains hypophosphite of soda daily. 10th. Weight 152 lbs., urine 2 days' average, 29·5, sp. gr. 1027, solids 796, U(H) 411; hypophosphite of soda with strychnia. 15th. Urine 2 days' average, 39, 1021, solids 819, urea (H) 486; constipated. 16th. Weight 151½ lbs. 24th. Weight 150 lbs., ½ pint milk added, diet 3·11, 2·07, 10·10=15·28. May 1st. Weight 150½ lbs., ½ pint more milk, diet 3·51, 2·42, 10·55=16·48. 6th. Urine 2 days' average, 44, 1013, solids 572, U(H) 286. 8th. Weight 152¼ lbs. 15th. Weight 151½ lbs. 17th. Urine 36 oz, 1028, solids 1008, urea (H), 504. 22nd. Weight 151½ lbs., urine 2 days' average, 28½, 1027, solids 768, urea (H) 397; had been purged by pills. The diet appeared now to be sufficient, but as the man was much below his former healthy weight I increased it again, and added 1 egg, 4 oz. bread, diet 4·10, 2·66, 12·63=19·39. 23rd. ½ pint beef-tea, 4·40, 2·66, 12·63=19·69. 29th. Weight 155 lbs.; 3½ lbs. increase in 7 days. June 1st. Average of 3 day's urine, 28·6, 1025½, solids 731, U(H) 394. June 3rd. Weight 156 lbs., 4 oz. bread struck off, diet 4·06, 2·62, 10·55=17·23; added 6 grains iod. pot. daily to medicine. 8th. Weight 155½ lbs. 10th. Urine 2 day's average, 24 oz., 1026, solids 624, urea (H) 333. 12th. Weight 158½ lbs. 19th. Weight 156 lbs.

Richards, coachman, twenty-six, height about 5 feet 6 inches; advanced phthisis; quick pulse. May 17th. Weight 124½ lbs., diet middle—½ pint milk, butter and sugar, cod-oil ʒii.=3·13, 2·90, 10·10=15·88; steady improvement in symptoms from second day. 30th. Weight 122 lbs., diet as above, with 2 oz. meat for breakfast, ½ pint more milk, and ½ pint beer—4·03, 3·65, 11·15=18·83. June 5th. Weight 124 lbs.

Patch, forty-five, convalescent from rheumatic fever, and walking about the ward; a tall, gaunt man; taking quinine, gentian, and iodine of potassium in small doses. February 27th. Weight 158 lbs., diet middle—beef-tea ½ pint, milk 1 pint, b. and s.=3·83, 2·96, 10·54=17·33. March 1st. Urine, 2 days' average, 66, 1017, solids 1122, U(H) 528. 2nd. Weight 162lbs., diet as above, with bread 4 oz.=4·27, 3·, 12·62=19·89. 4th. Urine, 2 days' average, 78, 1017, solids, 1326, U(H) 624. 6th. Weight 162¼ lbs. The additional bread seems to have been worse than useless, the whole of its nitrogen having gone to augment the urea.[a]

Brown, seaman, thirty-two, middle stature; melancholico-phlegmatic temperament; dormant phthisis; slow pulse; taking iron and quinine. January 30th. Weight 133 lbs., diet middle—bread 6 oz., beef-tea 1 pint, b. and s.=3·84, 2·33, 12·76=18·93. February 9th. Weight 138 lbs.; 5 lbs. in 10 days, a considerable increase on a moderate diet.

Hale, thirty, painter, small man; convalescent from painter's cholic; taking 6 grains of KI per diem, with gentian. October 17th. Weight 106 lbs., diet middle, not much b. and s.=say 2·71, 1·70, 9·35=13·76. 21st. Urine 48, 1012, solids 576, U(H) 283. 22nd. Urine 52, 1014, solids 728, U(H) 353. 24th. Weight 105½ lbs.; went out. November 7th. Weight 112½ lbs.; had returned to hospital to be weighed; his clothes were nearly the same. In this case the poor man had not got, while in the infirmary, a sufficient diet, as no other reason, that I could make out, would account for his subsequent rapid increase.

Jolly, seventeen, well-grown boy; recovering from second attack of acute rheumatism; quick excitable pulse; supposed to have had pericarditis in first attack; quinine; walking about the ward. January 14th. Weight 119½ lbs., diet middle, milk 1 pint, beef-tea ½ pint, b. and s.=3·83, 3·, 10·54=17·37. 16th. Urine 2 days'

[a] Roughly, 3 parts of nitrogenous food produce 1 part of urea. For albumen is to its contained nitrogen as 6·33 to 1; and urea to *its* nitrogen as 2·14 to 1; and 2·14 : 6·33 very nearly as 1 to 3. The rule is not new, but is not generally known.

average, 54, 1021, solids 1134, urea (H) 673, weight 121 lbs. 19th. Urine of 2 days' average, 62, 1019, solids 1178, urea (H) 719, weight 122¾; went out. February 2nd. Returned to weigh, weight 125 lbs. It is difficult to understand how this lad could have been increasing in weight, and at the same time passing so large a quantity of solids. There was some reason for suspecting an error (but not a large one) in the specific gravity.

A slight attack of pneumonia, and speedy convalescence, afforded me an opportunity of experimenting on myself. I had, however, no weighing machine at hand. I found that with a diet of about 5· nitrogenised, 3·75 fatty, and 11· amylo-saccharine, or nearly 20· altogether, consisting mainly of eggs, milk, &c., I was passing as little as 41 oz. of urine daily, of sp. gr. 1020, yielding by estimate 820 grains of solids.

Dr. Chambers quotes the case of a patient of his, a girl, who had gained 4 lbs. in six days, "on the sole allowance of 3 pints of milk, and less than 12 oz. of bread, per diem." This is a good rate of increase, but the diet stated would yield 3·42 N, 2·22 F, 8·94 Am, = 14·58, which would be a large diet for many girls. The nutritive powers of milk are generally under-estimated. The milk diet of an hospital is generally supposed to be much inferior in this respect to the meat diets, but this is not always the case, as may be seen by a reference to the foregoing tables.

It is obvious that these experiments ought to be much extended before one could draw any very positive conclusions from them; and, moreover, that if sufficiently followed up and made more precise they might be applied to the determination of the necessary quantities of food for the support of the bodily functions, with much more probability of success than the ordinary plan of calculating the excretions alone, which has been already practised to a great extent.

At present, all I wish to base upon them, is the opinion, that a convalescent hospital patient may often consume a very large quantity of food, without any, or without a commensurate increase of the urinary solids, but with considerable gain of flesh. In such a case I maintain that it is true economy to add largely to the ordinary diet, for by so doing the patient's sojourn in hospital may be considerably abridged, his power to work may be sooner restored to himself and the public, his bed will be sooner available, and the finances of the hospital will not, *quoad* the individual patient at least, be at all more drawn upon. I by no means intend to affirm,

however, that for the ordinary run of cases, where patients are not much emaciated, or reduced in muscular power, the diets usually employed, *e.g.* the middle diet of the Bristol Infirmary, are at all deficient in quantity: on the contrary I have found that patients of that class, especially men of small frame, appear often to thrive upon them. The estimate of what Dr. Playfair calls a "subsistence diet," for a man without exercise, which he makes in his valuable lecture "On the Food of Man, in Relation to his Useful Work" (published since this paper was written), is to some extent confirmed by this last observation, and Dr. Inglis's testimony to a similar effect, is of value, as the diets at Worcester Infirmary approximate very closely to Playfair's standard. Returning to the subject of convalescents, I would again draw attention to the very large proportion of their food which, when they are liberally dieted, is stored up by them in increase of bodily weight. Pigs, according to Lawes and Gilbert, appropriate far more (nearly double) from their food than sheep and oxen; and they seem to suppose this difference to depend on the greater digestibility of their food, as much, or more than an inherent difference of constitution and digestive power, so far as the two causes are separable. Now a fattening pig, they say, will consume daily about 4 per cent. on his live weight, of the dry substance of his food, and will yield 1 part of increase of live weight for nearly 5 parts of that dry substance: *i.e.*, a pig weighing 100 will consume 4, and add 0·8 to his weight, in one day. Only one of my men, Powell, added to his weight at a greater rate than this; but on the other hand, neither he nor any one of the others consumed more than $1\frac{1}{2}$ per cent. of his weight daily, in dry aliment; so that the human convalescent gave a vastly greater return for his food than the fattening pig, a fact which might have been of great interest in the Figi islands in the last generation, as proving the economic superiority of the "long pig." I attribute this difference, in a small degree only, to the superior character of the food of the men (for milk and meal will fatten men as well as swine), but chiefly to two causes, viz., the peculiar activity of the assimilative organs induced by the wasting and semi-starvation of an acute illness overpast, and the different chemical constitution of the increase in the two cases. That of the fattening pig consists mainly (65 or 70 per cent.) of dry fat, while in the convalescent man it doubtless comprises much muscular tissue, blood, &c., which include a very large proportion of water. It is

only on this last supposition that the rapid increase in such cases as those of Powell and Williams can be accounted for.

One point of some practical importance still remains to be noticed; I mean the necessity, which indeed is generally recognized, of allowing a very copious diet to consumptive patients. The cases of M'Carthy and Richards exemplify this pretty clearly. Phthisicals are with difficulty admitted into many hospitals; but where they are so, it as foolish as it is unjustifiable to withold from them as much nourishing food as they can digest.

Art. V.—*Ophthalmoscopic Notes.* By Henry Wilson, F.R.C.S., &c., Assistant-Surgeon to St. Mark's Ophthalmic Hospital.

RETINITIS PIGMENTOSA.

Among the manifold discoveries made in the living eye since the introduction of the ophthalmoscope into practice, that of pigmentation of the retina, made by Von Graefe some years ago, is one of the most interesting. It is, no doubt, true that pigment was found and described in the retina by Von Ammon, Walther, and the now venerable Mackenzie, long before the ophthalmoscope was invented, but only in morbid specimens, in which it appears to have been regarded as a mere accidental peculiarity. Although, however, they were unaware of the true nature of the phenomenon, or of its occurrence as a disease, yet I venture to believe that the earliest recorded instances of retinitis pigmentosa are to be found in the writings of Walther and Mackenzie, the latter of whom relates, as an instance of congenital night-blindness, the case of a deaf and dumb boy, which, from its history, symptoms, and *post mortem* appearance, was, I have no doubt, an example of congenital retinitis pigmentosa.

Pigmental degeneration of the retina, styled by the French, *rétinite tigrée*, originally termed *morbus Arianus*, after a nobleman in whom the disease was remarkably developed, and now universally known as *retinitis pigmentosa*, consists, as its name denotes, in a deposition of pigment in, and degeneration of, the structure of the retina. Strictly speaking, however, the latter name is, I think, not always accurately correct, for it implies and conveys the idea of an isolated, independent, and active disease in the retina, whereas it usually occurs as the result of inflammatory

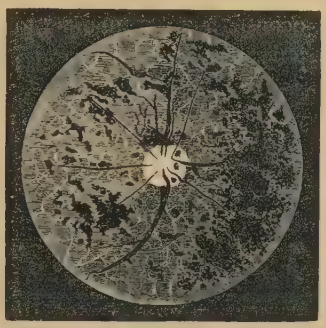
processes elsewhere, and the retina may be merely passively and secondarily engaged, as I shall mention presently. Independent development of pigment may and does undoubtedly occur in the retina without any other structure being implicated; but this appears to me to be the exception.

Eyes affected with this malady present externally but little peculiarity, except in the very advanced stage of the disease, when the pupil becomes very small, and responds but slightly to the stimulus of light, or in children when nystagmus occurs—an oscillating or to-and-fro movement, common to all congenital or early acquired ocular defects. One of the earliest symptoms generally complained of is night-blindness, at first partial, and in the advanced stage complete. Vision then begins to fail by day, and in such a manner that the field of vision is narrowed. There is, also, loss of the power of fixation; the ability of at once and promptly discerning any given object is diminished, and the patient sees best in strong light. There is no pain, nor is there—externally at least—any trace of inflammatory action. The disease is an exceedingly slow and insidious one, and terminates, as a rule, in blindness. It is essentially progressive, but continues sometimes as long as twenty years before terminating in complete blindness. It occurs congenitally, is hereditary, and is frequently complicated with opacities of the lens and in the vitreous humour. The ophthalmoscopic examination reveals a very marked contrast to the normal state; the fundus of the eye, instead of being of a uniform brilliant or dull red (according to the pigmentation of the choroid) presents patches or spots of a jet black colour, varying in shape and size, scattered irregularly over it. These spots are generally stellate, sometimes small isolated black specks, irregular in outline, having more or less numerous projecting processes, from which appearance they have been likened to bone corpuscles; or they occur as jagged irregularly shaped striæ; or grouped and massed together, forming a black irregular network. The black deposit is nearly always in greatest quantity at the place of division of the retinal vessels. These latter are, as a rule, extensively implicated in the disease, and present here and there streaks of pigment along their course, while at other places they are wholly or partially obliterated, or appear as very fine thin threads, which condition is usually best seen on the optic papilla. The optic nerve is generally somewhat atrophied, and has occasionally some isolated specks of pigment on its intraocular surface. Liebreich has observed a unique case of

pigmentation of the entire papilla, in which the deposit was confined to the optic disc, without any trace of it elsewhere in the fundus; it was, however, impossible to decide whether this pigmentation was a congenital anatomical peculiarity or of a pathological nature. Pigmentation of the retina commences usually at the peripheral and equatorial parts of the retina, and advances slowly, but surely, towards the optic disc.

The black deposit cannot well be confounded with accumulations of pigment which are sometimes seen in the choroid; that it is situate in the retina is ophthalmoscopically recognizable, from the circumstance of its being either in the same plane as the retinal vessels, or even anterior to these, so that they are concealed by it, as well as from the fact of the choroid being generally distinctly visible behind it. In the large majority of cases the choroid itself is extensively diseased, and its pigment epithelium so highly atrophied that the large choroidal vessels are clearly visible. In these cases the black specks, and masses of pigment in the retina, form a very marked contrast to the bright yellowish or red colour of the underlying choroidal vessels, and the dark red of the choroidal intervascular spaces. This condition can be very inadequately represented in anything but a coloured plate. I have, however, attempted to delineate it in the accompanying wood-cut, admirably executed by Mr. Hanlon, from a drawing I made of the fundus of the eye of a man aged 55, who stated that he enjoyed excellent vision up to the year 1862, when he noticed that he could not see as clearly as usual in the twilight. His vision has gradually decreased since then, both quantitively and qualitatively, so that when he came under my care in Spring he could only read, and with very great difficulty, "canon" type (No. 18, Jæger). His field of vision was small, and it took him a considerable time to make out the exact outline of an object. He saw best in a bright light; the head was held erect, and the eyes appeared as if seeking the light—just the reverse of the condition seen in persons affected with uncomplicated cataract. About the time he noticed his vision becoming impaired he had suffered great domestic affliction and much anxiety. At this time, too, he had a feeling of great heat, and tightness in the head, and would sometimes stagger in the streets like a drunken person—symptoms probably referable to intracranial disturbance. The pupils were normally dilated, but sluggish; the lenses slightly muddy, the left having a few striæ at its circumference; the vitreous was clear and free from muscæ.

The ophthalmoscopic examination revealed the condition I have endeavoured to illustrate by the subjoined wood-cut, which represents the enlarged reversed image of the left eye.



The background of the image is formed by an unequally coloured structure, composed of bright yellow and red tortuosities, which vary in size, and possess well-defined outlines, with dark coloured island-like interspaces indicated in the cut by the white lines and shaded interspaces, which give but a faint idea of the beautiful yellow plexuses formed by the choroidal vessels, which have become visible in consequence of complete and extensive atrophy of the layer of choroidal epithelium. The colour is much lighter at the position of the yellow spot—to the left of the central disc in the cut—where the atrophy has not only destroyed the pigment epithelial layer, but has engaged the stroma of the choroid. In front of this background, that is nearer to the observer, a quantity of jet black patches and specks are scattered irregularly over the fundus; these are aggregated here and there into net-like masses, especially on the outer or temporal side; one large triangular-shaped mass is seen encroaching on the upper border of the optic papilla, and three smaller ones are seen on its surface, towards the temporal side. The retina appears very slightly hazy on the inner side, and around the papilla; the papilla itself is somewhat atrophied, of a greyish white colour; its outline is slightly indistinct and irregular, indented here and there. The condition of the retinal vessels is very peculiar; above and below a large dark-coloured gorged vessel arrives from the periphery at the optic disc, after entering which it is seen to become much smaller, to assume, apparently, a different direction from that which it previously possessed, and to taper gradually to a point — an appearance somewhat similar to the

condition known as excavation of the optic nerve. Besides these there are several small arteries, some of which are so minute and attenuated that they can only be seen by using a high magnifying power; some are altogether obliterated or disappear in the network of pigment. Pigment is seen along the coats of some of the vessels. The pigment is in the same plane or in front of the retinal vessels, and the denuded large choroidal vessels are distinctly visible behind it.

Opinions are still divided as to the origin of this very peculiar disease. Donders, who was the first to institute a careful microscopical examination of the structures engaged in it, considers it to be a special and independent affection, combined with, or due to, a chronic inflammatory process in the retina; his view as to the idiopathic development of pigment in the retina has been confirmed by a case in which Schweigger, of Berlin, found on dissection the choroid perfectly healthy and normal in its entire extent, while the pigment occupied a zone of the retina lying between the equator and the ora serrata, and was confined to the vessels. Other investigators have arrived at the conclusion that the pigment is derived from extra retinal sources; thus Schweigger, who has devoted considerable attention to the pathology of ophthalmic diseases, and who has published the best and most practical work on the use of the ophthalmoscope, maintains that in the majority of cases pigmentation of the retina is attributable to choroiditis, with effusion and secondary infiltration of the retina. A very strong argument in favour of this view is afforded by the observations made independently by Schweigger and H. Müller, that at these places where the retina was unaffected and perfectly healthy, the subjacent choroid was likewise perfectly normal and unaltered, while at these spots where the retina was implicated in the disease, the choroid was correspondingly altered, and was devoid of its pigment cells. Some ophthalmologists still regard pigmentation of the retina as a mere accidental circumstance, which may ensue on any form of retinitis. In the case I have cited it would seem from the condition of the vessels that there had been an independent and idiopathic development of pigment in the retina, while the present condition of the choroid points unmistakably to the existence of choroiditis, so that the pigmentary deposit may be attributed to both causes—intra and extra retinal.

Our knowledge of the causes and mode of origin of pigment as a pathological product is, I think, still a limited one. Melanotic

formations occur in various tissues and secretions, as in pleural adhesions and in sputa; the small cerebral arteries are also occasionally found to have undergone pigmental degeneration. Paget states, with reference to this subject, that the general tendency of inflammatory products is to imitate the properties of the natural products of the part inflamed; and so far as disease of the lungs, choroid, and iris are concerned, this explanation is verified; but whether it will be found satisfactory in all cases of pigmental degeneration is doubtful.

The pigment is found, on microscopical examination, to be principally in the layer of grey nervous matter, and on the vessels. It occurs, however, in the various layers of the retina. It is found as pigment granules, or as pigmentated cells, which cells are flattened, polygonal, discoid, or elongated, the larger polygonal ones having a bright nucleus. One or more of the retinal laminæ is, as a rule, atrophied. From its proximity to the choroid, the bacillary (Jacob's) layer is the most frequent to suffer, and is generally highly atrophic. The walls of the vessels are found to have undergone a hyaline thickening whereby the tube is narrowed or even obliterated.

A remarkable feature in this disease is its hereditary nature, and its occurrence in the children of consanguineous parents. In thirty-five cases, which Liebreich noted more particularly, he was able to prove descent from blood relations in nearly half. Thus, another example has been added to the list of ocular and other defects dependent on consanguinity, to which Sir William Wilde has drawn attention in his writings, more especially in his essays on Deaf Dumbness and on Congenital Malformations of the Eye, published many years ago in this Journal.

I know of no special or general plan of treatment which will affect this disease permanently; attention to and improvement of the general health, and alteratives will, at first, usually produce a marked change for the better; but this beneficial result is merely temporary and fleeting, and after some time vision will return to its former condition, and the disease will advance steadily, until eventually the patient is hopelessly blind. Opacities of the lens and in the vitreous humour frequently supervene.

Illustrations of this affection are given in Plate 6 of Liebreich's magnificent *Atlas der Ophthalmoscopie*, in Plates 6 and 8 of Jæger's *Beiträge zur Pathologie des Auges*, in Plate 79 of Sichel's *Iconographie Ophthalmalogique*, in Plate 19, Figs. 9 and 10 of Ammon's

Klinische Darstellungen der Krankheiten des Menschlichen Auges, in Wedl's *Atlas der Pathologischen Histologie des Auges*, &c.

DISLOCATION OF THE LENS.

Luxations of the transparent or opaque lens, some varieties of which I described in the *Ophthalmic Hospital Reports*, are not of uncommon occurrence, and have probably been seen by the majority of practitioners. I will, therefore, now merely mention a peculiar phenomenon which is sometimes noticed during the ophthalmoscopic examination of such cases, and which may not be generally known. In certain abnormal positions of the transparent lens the observer is startled by seeing *two* optic nerve entrances instead of one. Some time ago a patient came under my care with hypermetropia of the right eye, and dislocation of the left lens. This person had received an injury of the left eye four months previously, and I found the lens dislocated inwards, in the vitreous chamber, with its equator nearly in the centre of the pupillary space, and still sufficiently transparent to allow of a good view of the fundus of the eye. The internal structures were considerably altered, the retina was hazy, and its vessels indistinct; the optic papilla was ill defined in every respect, but still visible. In the reversed image two optic discs were visible, one to the temporal side and the other to the nasal side of the fundus. These could be made to coalesce almost entirely, immediately behind the equator, by lateral movements of the convex object lens. The cause of this phenomenon was that one image was projected by the lens and refracting media, the other, on the contrary, by the refracting media alone, without the assistance of the lens; and the dislocated lens caused the image projected by itself to occupy a different position from the one in the production of which it had no participation.

ART. VI.—*Case of Fracture of the Spine, in which the Operation of Trephining was Performed, with Observations.* By ROBERT M'DONNELL, M.D., F.R.S.; Fellow of the Royal College of Surgeons; Surgeon to Jervis-street Hospital; Lecturer on Surgery in the Carmichael School of Medicine; Examiner in Surgery in the Queen's University.

UNSUCCESSFUL cases are often more instructive to the practical surgeon than those which terminate favourably; with reference to the subject which I propose to discuss in the following pages this is

strikingly true, for I cannot help believing that thoughtful persons will be led to look favourably upon the operation of trephining the spine in certain cases of injury, as much from a careful consideration of some of the cases in which this operation has failed to save life, as from the account of those in which life has been actually saved by it.

It must be admitted that the question as to the judiciousness of trephining the spine, with a view to remove pressure from the spinal marrow, is one of the nicest which can be presented to the surgeon; and we know that surgeons of the greatest eminence are to be found ranged on both sides of the controversy—a controversy which, I regret to say, has not always been carried on with the calmness which should characterize such debates.

Before myself determining to recommend this operation, or to perform it if a suitable case should occur in my practice, I had very carefully studied what has been written both for and against it, and, so far as was possible, I had examined accurately the details of the published cases in which the operation had been performed. After due deliberation I had come to the conclusion that even on *purely theoretic* grounds, and viewed by the light which modern physiology and physiological experiment throws upon the subject, the weight of evidence lay on the side of those who, with Sir Astley Cooper at their head, advocate the operation. But in the advancement of surgery everything is essentially tentative, and results of cases must be the true criteria by which the most plausible theories are to be tested. An analysis of these results appears to me to lead irresistibly to the conclusion that in many instances this operation is good surgery; but in attempting to form a just appreciation of the value of the results of these cases, I do not intend to refer simply to those in which life seems to have been saved by the operative procedure; in not a few of the cases, which ultimately ran a fatal course the improvement which followed the operation was at least sufficient to show that the removal of pressure from the spinal cord is the best method which can be adopted in order to try to break the chain of sequences which, when uninterrupted, leads the patient suffering from fractured spine so inevitably to the grave. The case which I have to detail belongs to this latter group:—

Joseph Collins, aged thirty-eight years, a thin, spare man, ordinarily employed at labour, was admitted to Jervis-street Hospital on December 28th, 1864. A short time before, while

working in the hold of a vessel, from which a cargo of wheat was being discharged, a sack of corn had fallen upon him from a height of seventeen or eighteen feet. The weight fell upon the back of his head, neck, and shoulders; he sank beneath it, and to use his own expression, was "doubled up." Immediately after the injury his lower limbs were powerless; he was at once conveyed to hospital, where I happened to be at the moment of his admission; I had, in consequence, an opportunity of examining his spine at the seat of injury before any tumefaction had occurred. I found that the spinous process of a vertebra, corresponding with either the first lumbar or the last dorsal, was more prominent than natural, while a marked depression, leaving no doubt whatever as to the existence of displacement between the vertebræ, was to be felt in the place of the spinous process of the vertebra above. When a circle was made round the body, at the level of the umbilicus, with a piece of cord, the prominent spinous process was found to be exactly four inches above this circle; it was accordingly fixed upon as being the spinous process of the first lumbar vertebra. The lower limbs were paralysed, as were also the bladder and rectum. The urine drawn off by catheter was not bloody. At a consultation on the afternoon of the day of the patient's admission, and again the following morning, I urged as strongly as possible the necessity for operation. I could not, however, induce the majority of my colleagues to take my view of the case.

It is hardly necessary to detail the progress of the case during the days which immediately followed; the loss of power of motion and of feeling became more marked than just after the injury; the urine dribbled away; the feces were passed involuntarily; on the ninth day the urine was neutral; on the eleventh alkaline, and containing copious muco-purulent deposit; a few days later it became bloody, and of a most offensive odour. Although placed on a water bed, and attended to most carefully, bed sores formed; the penis became ulcerated, and the scrotum œdematous, and of great size. He suffered from thirst, flatulence, and pains over the bladder.

During the last days of January my friend, Dr. Brown-Séquard, happened to arrive in Dublin; he saw the patient with me, and thought that even still operation offered a chance for life, instead of certain death; the majority of my colleagues agreed, and with the patient's consent, indeed at his urgent request as soon as he understood the hopeless nature of his case, the operation was determined upon.

I now copy from my note book the details of the case from day to day.

Condition immediately Previous to Operation.—Dr. Brown-Séquard arrived in Dublin on January 30th; the following morning he accompanied me to visit the patient. He was then carefully examined, and was found in the following condition:—Pulse 100, regular, but feeble; tongue clean, rather dry; had rested tolerably well the previous night; no headache; complains of flatulence, and has a tendency to diarrhea; bladder completely paralysed; urine constantly dribbling away; no urine accumulates in the bladder; he is absolutely unconscious of the discharges passing "per anum;" and fluid feces ooze incessantly from the rectum; the penis is swelled, and the prepuce ulcerated from constant contact with the urine; there is an ulcer as large as sixpence at the root of the penis, in the fold between the penis and scrotum, and the whole scrotum is swelled, red, and superficially excoriated; back, over the sacrum, ulcerated to a considerable extent, but, except in one small part, not deeply; at this one part matter can be pressed out, showing that the ulceration extends to some depth under the skin. Upon the inner ankle of left foot, and the outer ankle of the right two smaller spots (dry and scabbed) exist, where ulceration has occurred apparently without pressure of any kind having taken place. Paralysis of motion is almost complete in both lower limbs; in this respect they are exactly alike. On doing his best to produce a movement in the legs, a slight motion is perceived in each groin; the muscles of the thigh above the knee, and of the calf, and of the toes are motionless; no reflex movement can be excited; sensation is normal along the thighs, over the skin of the calf and shin; rubbing or pressing on the sole of the foot is not perceived; he can, as regards the thigh and calf, distinguish the compass points at the usual distance, as in persons who feel normally; he can tell what part of the thigh or calf is touched by the hand, and in these parts distinguish, with precision, heat and cold, and also pricking with a point. There is no difference in these respects between one limb and the other; in each foot sensibility is much impaired, and the sole of each is devoid of feeling altogether.

Operation.—I operated on February 3rd, 1865. My colleagues, Drs. Hughes, Stapleton, Banon, Tyrrell, and Forrest were present. Dr. Brown-Séquard was also present, and many surgeons of Dublin.

The patient was not removed from the ward or from the bed on which he lay. This was in order, as far as possible, to avoid disturbing any callus which might possibly have already formed. He was put under the influence of chloroform as he lay on his back in his accustomed position; when chloroformed, the bed was carried opposite to the window. The patient was turned over on his face; in this position the seat of the injury was obvious from the prominence of the last (?) dorsal vertebra. A small spot of ulceration, not so large as a sixpence, existed over the most prominent spinous process at this part.

An incision, nearly five inches long, was made with a strong scalpel over the spinous processes of the vertebræ (the two last dorsal and two first lumbar); then, with a strong curved bistoury, the slips of tendon on each side of the spines were divided. Keeping quite close to the bone the mass of muscle was detached on each side, and securely held apart by broad retractors, well suited for the purpose. The spinous processes, and laminæ, in fact, the back of each vertebra as far out as the articulating process was thus fairly and fully exposed. The spine of each exposed vertebra was then taken hold of in a strong pair of necrosis forceps, and cautiously but firmly shaken to try whether any fracture of the posterior arch, or of the processes could be detected. No such fracture existed. (This I expected, as I had reason to suppose from the nature of the injury, that if any fracture existed it was one of the body of the vertebra).

On examination I was satisfied of the displacement existing in the parts with which I had to do; it was as follows:—The last dorsal vertebra (?) was as it were twisted, so that on the left side the articulating process was raised up, and although not completely dislocated, yet it stood prominently backward from the corresponding process on the bone below; on the right side the superior articulating process of the same vertebra was in exactly the opposite predicament; it was displaced so as to be as if pushed in deeper than the process to which it corresponded on the vertebra above.

I determined to remove the spinous and inferior articulating processes of this vertebra, by cutting through the laminæ. After waiting for a time while sponges and cold water were applied to stop bleeding, I first took off a part of the spinous process of the part I was going to remove, and divided the interspinous ligaments above and below. I next, with a strong pair of bone forceps, cut through the lamina on the patients left side (on which side I stood

myself). This, owing to the displacement I have already described, was quite easily effected, as, of course the lamina, as well as the articulating process, was raised up somewhat on this side. On the right side, however, the opposite state of things made it much more difficult to succeed in cutting through the lamina. I could not succeed at first with the cutting forceps, but was obliged to use a Hey's saw guarded, so that it could not go in beyond a certain depth.

I was unwilling to use the saw, lest the unavoidable shaking might injure any callus already thrown out, and tend to undo any repair that nature had already commenced.

I finally succeeded in dividing the lamina on the right side with the forceps, I then grasped, in a pair of necrosis forceps, the root of the spinous process of the portion thus separated, and raising it cautiously, divided, with a scalpel, the ligamentous structures which now alone prevented its removal. On sponging away the blood, the theca vertebralis came into view; a small portion of the arch of the vertebra above was also removed with gouging forceps. The spinal cord was obviously pushed backwards, and had lain very close under the arch of bone taken away. The vertebral theca was not tense; there was no evidence of either blood clot or fluid being pent up within it; it was therefore not opened, although I had at first intended to do so.

Slight venous hemorrhage took place from the veins underneath the bone.

The operation lasted about an hour; it was protracted by long rests now and then, waiting until the sponging with cold water and infusion of matico checked oozing of venous blood, as it was necessary to see with great precision what one was doing. I do not think that more than five, or at the most six, ounces of blood was lost. A couple of sutures were put in at the upper part of the wound; the lower part was left open, a small tent of soft sponge alone being introduced.

Notes immediately Subsequent to Operation.—The patient was replaced upon his back, pads of spongio-piline being placed above and below the wound, and the sacrum protected by a circular cushion. He lay on a water bed, nearly flat, the head but slightly raised, and a cushion being passed behind the knees. He got an opiate enema; while being arranged a jet of urine was thrown from the orifice of the urethra; of this he was not conscious.

The same evening he commenced taking the 96th of a grain of atropine in solution, to be continued three times a day.

The bladder and the large intestine were carefully washed by injections of tepid water and thin flaxseed tea.

February 4th.—Day after operation; pulse 120, regular but weak; had rested tolerably, sleeping for a couple hours at a time, some headache, and heat of head and skin; tongue dry, but not coated. When about to wash out the bladder, the urine was observed to come in a jet from the orifice of the urethra; bowels had not acted since the previous day, the opiate enema having checked the diarrhea; the pupil was not affected ; the atropia was continued in the same dose as before; the penis and scrotum diminished in size, and the superficial ulceration better. In the afternoon, Dr. Brown-Séquard accompanied me to see him; we then found that sensibility had returned in the soles of the feet, and that a decided return of motor power had taken place in the muscles of the thigh. Complains of cough, which hurts him—in fact, he had caught cold from the exposure during the operation. Ordered iodidi potassii three grains, in decoct. cinchonæ flavæ, along with the atropia.

5th Feb.—Pulse 108. Skin warm, but natural; tongue moist; had rested tolerably; no headache; appetite returning; had an egg for breakfast, and asked for a mutton chop for dinner; cough much less troublesome. Dr. Brown-Séquard again this day examined him along with me; we observed some œdema of the left leg and foot; sensibility is now almost, if not quite, normal all over the foot and sole. The sartorius, hamstring, and quadriceps extensor femoris muscles are able to contract with considerable strength; we can perceive no sign of movement in the muscles of the calf, or in the toes; their motor power is still absolutely wanting. Marked improvement has taken place in the state of the penis and scrotum. I dressed the wound and the sore on the sacrum, causing the patient to be raised right up by five assistants; after the application of the dressing, he was replaced on the circular cushions as before; healthy pus coming from the sore over the sacrum. When he coughs he feels pains across the back. Bladder and rectum washed out as before.

6th.—Pulse 100. Tongue moist; skin natural; altogether appears to be going on satisfactorily; has had a free evacuation from the bowels of natural appearance and consistence. He states distinctly that although not able to control the passing of the evacuation, he was conscious of its passage.

The bed clothes underneath the patient were entirely changed; the wound and sore on sacrum dressed; the latter looking clean, and secreting healthy pus. Hitherto I should say that the most marked improvement which I can notice in the patient is that in the condition of the sores on the sacrum, and those on the prepuce, penis, and scrotum.

Atropia continued, with decoct. cinchonæ and iodid. potassii. Eats an egg at breakfast, a chop, with porter, for dinner, and an egg in the afternoon. Bladder washed out several times a-day.

7th.—Appears to be making progress satisfactorily. I examined the condition of the lower limbs this morning, in presence of my colleagues, Drs. Lyons and Forrest; Dr. Beatty was also present, and the class. I think I may say that sensation is now normal everywhere; he can tell (without being allowed to look at the part touched) when the finger is gently applied or rubbed over any part, even of the sole; he distinguishes heat and cold in the sole as well as elsewhere. The motor power of the muscles of the thigh is much increased; the patella is moved with some force; as yet the muscles of the calf and toes are incapable of movement. Diet as before; atropia slightly increased; iodide of potassium and bark continued. Washing of the bladder continued several times daily.

8th.—Had a good night. Pulse 100; sores looking well; œdema of left foot nearly gone. Wound and sore on sacrum carefully dressed; bladder washed. Atropia has decided effect on the pupil. No observation made this day on sensation and motion of lower limbs.

9th.—Rested very well. Bowels freely moved without control, but with consciousness of the movement; penis and scrotum much reduced in size; œdema of left leg nearly gone. No particular examination of motion and sensibility made this day. Wound suppurating, and apparently going on favourably.

10th.—Had not a good night, yet does not seem worse as regards his general condition. Pulse 108; tongue clean and moist; bowels not moved during last twenty-four hours. For the first time to-day I observed distinct reflex movements of the muscles of the thigh, on touching or plucking the hairs on the inside of the thigh; in other respects as before. Atropia continued. Cough is rather troublesome; he attributes his restless night to this, not to pain; he has no referred sensations of pain in the lower limbs.

11th.—Pulse 108, and in other respects the same as yesterday; well-marked voluntary movements in all the muscles of the thigh;

none in the calf or muscles of the toes; no reflex movement in the calf or toes from touching or tickling the leg or sole. Reflex movements in the sartorius and adductors are to-day well marked, when the hairs on the inside of the thigh are plucked. Scrotum and cremaster also give reflex movements. The condition of the penis and scrotum much improved. The wound was dressed to-day; a copious discharge of pus, dark with blood. Bed sores on sacrum improved, and discharging healthy pus.

12th (Sunday).—After being dressed yesterday he had a severe rigor; this may be due to the almost unavoidable exposure to cold while the cloths underneath him are being changed. Bowels moved this morning; feces natural in appearance and consistence.

13th.—Pulse 100, feeble; sickness of stomach came on last night; is afraid to eat for fear of bringing on vomiting again. I injected the bladder to-day with turpentine ʒi, made into an emulsion, with yolk of egg; it produced no pain. Atropine is still continued; iodide of potassium stopped, as he fancies it causes sickness. Wound dressed to-day; edges very red.

14th.—Passed a good night; is altogether better to-day; pulse 100, and stronger; vomiting ceased; took his egg this morning.

15th.—Passed a quiet night; sleeps a good deal; bowels moved to-day; no material change since yesterday. The bladder is washed out daily with the turpentine emulsions, and I think I may say that the condition of the urine washed out indicates some improvement in the state of the bladder. The urine is certainly much less offensive, and the mucus, which was at first increased by the turpentine, is now diminished in quantity. Wound dressed; a large discharge of healthy matter; the bed sores are clean, and in a healing state.

16th.—To-day, for the first time, there is a marked improvement in the power of expelling the urine, on trying to do so.

17th.—Pulse 120, weak. Passed a quiet night, free from pains; he sleeps a good deal both by day and night; can now drive the water out of the bladder with a considerable degree of force. Motion, sensibility, and the reflex phenomena are in the same condition now for some days, neither getting better or worse. Scrotum quite pursed up, and of natural dimensions; penis parboiled, and swollen round the prepuce, but of natural size in other respects; bed sores going on well; wound secreting a large quantity of matter.

18th.—Had a rigor yesterday after being dressed; sickness has returned to-day; brandy and water freely given.

19th (Sunday).—Pulse 125, very weak. Felt very weak to-day when being dressed. Bowels moved; discharge thin, and indicating diarrhea; sickness continuing, not checked by ice, hydrocyanic acid, &c. Bladder has been washed daily with the turpentine emulsion as before. I believe the condition of the bladder to be certainly improved, and the voluntary power over it so likewise. He says he did not sleep at all last night, but does not know what kept him awake as he had no pain. The urine continued alkaline throughout.

He died rather suddenly on the morning of the 20th, having been seen by the resident pupil about an hour before, and reproved for smoking, which he had been found doing by the night nurse. He was then apparently quite himself, and not in pain.

Post mortem examination, made the same day (20th), in presence of the pupils. Body much wasted; no œdema.

Head.—Considerable subarachnoid effusion, also some clear serum in each lateral ventricle; brain structure and its membranes healthy.

Chest.—Lungs healthy, indeed remarkably so; no old adhesions; heart normal; no fluid in pericardium.

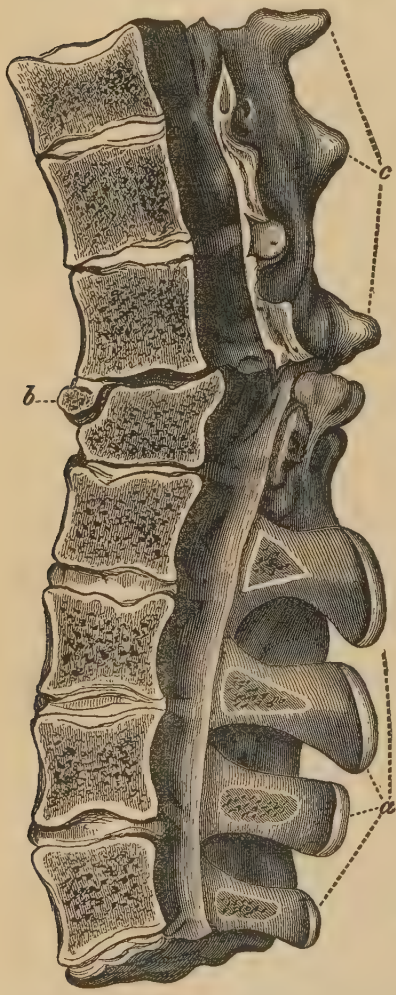
Abdomen.—Stomach and intestines healthy; no lodgement in any part of bowels; no ulceration in the rectum or other parts of the large intestine.

Urinary bladder contracted to a small size, greatly thickened, and containing small collections of pus in its walls. Mucous membrane ulcerated, and covered with ash-coloured shreds of adherent membrane; both ureters thickened, so as to be as thick as my little finger; the mucous membrane lining each was in the same condition as that of the bladder, and on the left side this extended all the way up to the kidney, the pelvis of which contained pus; its structure was disorganized by abscesses.

On examining the bodies of the vertebræ on their anterior aspect there was no inequality which made it obvious where the injury had taken place. The lumbar and lower dorsal vertebræ were removed; the spinal cord and its membranes were taken out by cutting through the laminæ of the dorsal vertebræ. Subsequently the section shown in the accompanying woodcut (Fig. 1) was made. The dura mater of the cord was uninjured; the portion of it corresponding to the piece of bone removed at the time of the operation was covered externally with lymph; the surface next the cord was healthy; there was no trace of inflammation within the dura mater. The cord was not inflamed or softened; it was indented at a point

corresponding to where the bone was displaced; and when the finger was passed along it gently it felt as if softened at this indention, but there was neither red or white softening of its structure, and this feeling was merely the result of pressure, which had not given rise to structural disorganization.

Fig. 1.



The body of the first lumbar vertebra was fractured, and this vertebra was displaced backwards; the line of fracture separated only a small portion of the body of the broken vertebra. The intervertebral substance between the last dorsal and first lumbar vertebræ had been torn, and the body of the first lumbar was displaced backwards, as shown in the accompanying woodcut. A small blood-clot occupied the space above the projecting body of this vertebra, lying between the body of last dorsal vertebra and the anterior aspect of the dura mater of the cord. The spinal cord was, therefore, pushed backwards by this effused blood, as well as the body of first lumbar vertebra, not simply by the sharp ridge of bone, as might be at first supposed from looking at the woodcut.

Fig. 1 represents a vertical section of the inferior part of the spinal column, as seen in the case of Collins (the lumbar vertebræ and the three last dorsal): *a*, the spinous processes of the four last lumbar vertebræ; *b*, the small portion broken off from the body of first lumbar vertebra; *c*, portions of the last three ribs. As the cord was pushed backwards, not only by the projecting body of first lumbar vertebra, but by the blood-clot filling the hollow above this projection, it is obvious that the complete removal of the posterior arches of both the last dorsal and first lumbar vertebræ would have set the cord free from pressure.

It was, of course, corresponding with this part that the indentation existed upon the spinal marrow.[a]

As a sort of appendix, at the conclusion of this communication I have given a table of reference to all the cases which I have been able to find out in which an operation has been undertaken to remove portions of bone pressing on the spinal marrow. These amount to twenty-six cases, in seven of which life has been preserved, viz.:—No. 1, Louis'; No. 9, Alban Smith's; No. 13, Edwards'; No. 16, Walker's; No. 17, Blackman's; No. 23, Blair's; and No. 24, Potter's (twice operated upon). Although it must be admitted that the details of these cases are in some instances meagre and unsatisfactory, yet the fact of the cases having occurred can hardly be denied. I have already said, however, that the improvement which took place in some of the cases which ultimately terminated fatally is a result which shows not only that good is in many instances derived from the operation, but that some of the arguments used against an operative procedure are not well founded. In my own case this improvement was well marked; it was evidenced by return of sensation, increase of motor power, return of the possibility of exciting reflex movements, but, above all, by the altered condition of the penis, scrotum, and bed sores. Tyrrell says after the operation in his first case:—"The patient could now feel distinctly on being pinched on the inside of the thigh, which *immediate return of sensation was beyond my most sanguine expectation.*" Mr. Wickham tells us—"The operation was easily performed, and the patient was in a degree relieved by it; his breathing became more free, and *sensation returned to a considerable extent.*"

In Holscher's case, eight weeks after the injury, sensibility returned in the feet; some week's later movement of the legs was possible. In Jones' case the amendment was so remarkable that I shall be pardoned for transcribing the case in full:—

"J. G., aged 34, a tall and remarkably muscular man, by trade a carpenter, was brought into the hospital, at 3 P.M., on the 28th February, 1856, in consequence of an accident he met with three-quarters of an hour before his admission. While employed on the deck of his vessel, his foot slipped, and he was precipitated into the hold, his head, at the same time, coming in contact with a block;

[a] These parts were exhiblted at the meeting of the Pathological Society, February, 1865, and are preserved in the Museum of the Carmichael School of Medicine.

he was taken up in a perfect state of insensibility, and shortly afterwards was seen by Dr. Marett, who accompanied him to the Hospital, where he was immediately attended to. It was evident that he then laboured under the effects of liquor, the breathing was rather stertorous; pulse 38, soft and feeble; pupils natural; excessive priapism. With the exception of a slight abrasion over the right eyebrow, there existed no sign whatever of external injury; the extremities were cold, so that hot water bottles had to be applied to them. When seen at 9 p.m. he had entirely regained consciousness, and complained of pain in the neck, and great uneasiness between the shoulders; also slight pain in the head; had sensation along the arms, but not in the fingers; there was entire paralysis and loss of sensation from the sternum downwards; breathing was consequently performed by the diaphragm alone. Pulse 54; no irregularity in the line of spinous processes was perceptible; the head was thrown backwards, and the integuments at the back part of the neck presented three or four folds running transversely. A strong cathartic was at once ordered, and ʒxvij. of healthy-looking urine was drawn off by means of the catheter. From this date up to the fifth evening after the accident no melioration took place; on the contrary, the patient slowly, but gradually, became worse; the catheter had to be introduced three and four times in the twenty-four hours, as all power of voiding urine was completely lost; the bowels were only acted on by means of croton oil; assisted by enemata; the pulse varied from 50 to 60; and, notwithstanding the precautions taken, a large sore on each of the nates had already commenced sloughing. On the 2nd March the sufferer was placed under the influence of chloroform, in order that a more minute examination of the spinal column might take place; the result was as unsatisfactory as the first. When seen on the 5th, at 9 a.m., a marked change for the worse had evidently taken place; a great tendency to lethargy, which had never before manifested itself, was apparent; and when roused, the countenance expressed considerable anxiety; the sensation along the arms was more deadened; speech thickened; and the pulse intermitted at every third and fourth beat. Under these most unfavourable circumstances, the following operation was performed, the patient having first been rendered insensible by means of chloroform:—

"An incision, about four inches in length, was first made on each side of the spinous processes of the third and fourth dorsal vertebræ, and were then connected together in the form of the letter H, by a

transverse one. As much of the muscular structures, together with their integuments, as the nature of the parts would allow, were then reflected upwards and downwards. The muscular mass on each side of the spinous processes, and that covering the vertebral arches, had to be cleared away, in order to expose those parts; this accomplished, the arches of the third and fourth dorsal vertebræ were sawn through close to their pedicles, and their interspinal attachments being loosened, were removed, so that the cord, covered by its membranes, was exposed; but little effusion of blood was found here, and no fracture could be detected; the incisions were, therefore, prolonged upwards, and, on exposing the lamina of the fifth and sixth cervical vertebræ, much ecchymosis was discovered, and the fracture detected. The same proceeding was adopted over these vertebræ as in the first instance, and the cord and its membranes exposed; the arches were fractured across; the membranes were not lacerated, but roughened; and there was much blood effused between them and the vertebral canal. After the removal of the fractured portions, the lips of the wounds were approximated by means of adhesive plaster, and water dressing, &c., applied. The hemorrhage was very abundant, though not serious, and chiefly of a venous character. The pulse after the operation rose to 80, and no longer intermitted. At 8 p.m. the patient had entirely recovered from the effects of chloroform; merely complained of smarting pain in the neck and back; was perfectly cheerful, and *had recovered sensation as low down as the umbilicus. The arms, too, which before the operation could only be drawn upwards, were now raised at pleasure, and could, without inconvenience, be thrown across the chest.*

"6th.—Slept comfortably during some parts of the night; pulse 70, and regular; sensation, as low down as the umbilicus, natural; has on several occasions felt the desire to void water, and has asked for the urinal two or three times; the power is, however, still wanting, and the urine has been drawn off. (It may here be stated that, from the time of the accident till the patient's death, this secretion was always sufficient, natural in character, and never once admitted the slightest ammoniacal odour.) On the 7th and 8th the improvement was gradual; and, at 1 a.m., on the 9th, I had some reason to hope that recovery might take place. I found he had slept well for five hours. He expressed himself as refreshed, was very cheerful, desired food, and gave it as his opinion that the catheter would not again have to be employed. Pulse was then 73, soft, and regular. At 5, his bowels were freely acted on from croton-oil pills, taken

the preceding evening. His linen was at once changed by the nurses, when almost immediately afterwards coma came on, and he expired at 10 o'clock the same morning. As the patient's relations were anxious to remove the body as soon as possible, the *post mortem* examination was of necessity a partial one, and, consequently, by no means as satisfactory as could have been desired. It showed the spinal cord to be entire and unlacerated in its posterior aspect. To the left side, opposite the pedicles, and passing obliquely across, and corresponding to the seat of the fracture through the bodies of the fifth and sixth cervical vertebræ, the cord bore the appearance of injury. Much blood was found in the neighbourhood of the fractured parts, and so complete was the injury and displacement, that the bones were preserved in their natural position almost entirely by the integrity of the anterior common ligament. The roots of the spinal nerves on the left side were in a great measure severed from the cord. There was considerable effusion at the base of the skull, and the muscles and structures on the back were congested."

I conceive that the amendment which was observed to take place in the cases to which I have now referred, offers the best answer to what has been urged as a cogent reason for non-interference in cases of fracture of the spine, viz., that as, in the great majority of instances of this injury, the fracture traverses the body of the vertebra, a displaced portion of which causes a diminution in the size of the canal, and as this portion is not accessible to an operation, it is argued that such a proceeding can be of no avail.

To give Sir Benjamin Brodie's[a] own words, "The question respecting such an operation seems to me to lie in a very small compass; if the whole or nearly the whole of a vertebra be driven forwards, the depression of the posterior part of it will, of course, occasion a diminution of the size of the spinal canal; but the removal of any portion of the vertebra, which is accessible to an operation, will be of little avail, as the irregularity in the anterior part of the canal, made by the displacement of the body of the vertebra, must be the same after, as it was before the operation." Mr. Alexander Shaw puts very strongly the same argument,[b] "Specimens," he says, "preserved in museums, prove it to be a

[a] Med. Chirurg. Transactions. Vol. xx., p. 160.

[b] A System of Surgery by Holmes. (Article—Injuries of the Back. By A. Shaw. Vol. ii., page 236.

general fact, that the fragments of the broken vertebræ, which are indented into the cord and commit the greatest injury, are in front, not behind. It does not appear, therefore, that benefit can possibly be gained, so long as the principal cause of compression continues to exist on the fore-part, by extracting portions of the posterior arches."

This line of reasoning has carried great weight with many; and I confess that it at first seemed to me the gravest of all objections offered against the operation in question.

The impartial enquirer will, I conceive, find the refutation of it simply in the facts shown in the cases already cited, which prove that benefit *has been gained* by extracting portions of the posterior arches; that motion and sensation have, more or less, completely returned, and ulcerations healed; in short, that the spinal cord has, to a certain extent, recovered its powers, and that, too, in cases in which the examination after death has shown the circumstances to be precisely those in which it would appear to Mr. Shaw that no benefit could possibly be gained. Sir B. Brodie's observation is, no doubt, quite true, that the irregularity in the anterior part of the canal, made by the displacement of the body of the vertebra, must be the same after as it was before the operation; but if the cord is pushed backwards, so as to be actually squeezed against the posterior arch, clearly the removal of the arch gives it room. Taking away the spinous process and laminæ in such a case as is figured in the accompanying woodcut (Fig. 2), would, without doubt, take pressure off the cord; and in my case the complete removal of two arches, as will be seen by looking at the woodcut (Fig. 1), would have set the cord completely free from pressure. The cord would, indeed, have still passed over an eminence, caused partly by bone, partly by the blood clot in front of its dura mater, but, the source

Fig. 2.

Fig. 2.—Showing a fracture of the body of a lumbar vertebra. The woodcut is copied from Dr. Gurlt's work on fractures (Handbuch der Lehre von den Knochenbrüchen von Dr. E. Gurlt), and is taken from a preparation belonging to Professor Middeldorp. It is obvious that in such a case, although pressure is caused by the fractured body, yet that it would be relieved by taking away the posterior arch.

of counter pressure being removed, we know very well that passing round an abnormal curve, even greater than this, does not prevent the spinal marrow from exercising its functions. I regret that in my case the posterior arch of the last dorsal vertebra was not taken away completely; it would have added little to the severity of the operation, and nothing to the danger, and would more thoroughly have taken pressure from the cord.

Although Boyer was opposed to the operation, yet one at least of the four cases which he adduces seems to give little support to the opinion which he forms.

"A sack of flour weighing three hundred pounds, fell on the nape of the patient's neck; an acute pain was felt in the lower part of the cervical region. The patient was conveyed to the Hospital of La Charité; on examining him I perceived that the spinous process of the seventh cervical vertebra was more prominent than in the natural condition. The upper and lower limbs were paralyzed; respiration laborious; the rectum and bladder incapable of acting. The patient died at the end of five days. At the *post mortem* examination we found a fracture of the posterior arch of the seventh cervical vertebra, with depression of a fragment which pressed on the spinal marrow, and exercised upon it firm compression."

The recorded cases of trephining of the spine show how groundless are the theoretic objections conjured up against this operation by Sir C. Bell; those persons, at least, who witnessed the operation in my case well know that he is quite in error in speaking of the great "degree of violence necessary for the performance of the operation," and those pupils who saw the patient's condition before and after the operation, and who watched his progress from day to day, will, I fancy, laugh at the ridiculous assertion that "the man must be already dead whose condition is not made worse by such an operation as this." Sir Charles Bell asserts that even if a sharp piece of bone were driven into the spinal marrow, causing paralysis of the lower limbs, that exposing the medulla to extract the fragment would so aggravate the mischief that inflammation, suppuration, and death, would be the inevitable consequences. This assertion experience has contradicted, and as a matter of fact, we know that the operation is not likely to be followed by inflammation and suppuration of the cord or its membranes. Indeed, as regards the occurrence of meningitis, it is natural to suppose

that in trephining the spine this would be less likely to occur than in trephining the skull, as in the latter case, the dura mater is very closely adherent to the bone; while in the former it does not constitute a periosteal lining for the bone, which is without difficulty detached from it.

Liston also speaks of the chance of inflammatory action being increased by operation, to which he is decidedly opposed. Mr. Alexander Shaw draws a terrible picture of the inflammation and suppuration which he fancies is likely to follow:—"Let any one," he says, "consider the ulterior effects which may be reasonably expected to follow, in general, from such a wound as must be inflicted in trephining the spine. The incision must be both long and deep, being made through the skin and the muscles, and through the blood infiltrated in the torn structures, down to the broken bone. The main object of the proceeding being to remove the fragments, and to lay open the vertebral canal, the external wound will communicate not only with the fractured surfaces but with the interior of the canal which contains the spinal cord. To the mind of the surgeon accustomed to witness the disastrous effect of compound fractures, the condition here described must appear the most formidable that can be conceived. He will look forward (if the patient should live) to profuse suppuration being soon established; to the pus penetrating extensively along the vertebral canal, between it and the membranous theca, to the exposed and the isolated ends of the fractured vertebræ becoming necrosed, and he will not expect that, until these be detached and cast off (a process requiring many months for its completion), the suppuration will cease or the wound cicatrise."

The reply which I would give to all this is very brief; it is simply this: within the present year I have seen this operation twice performed in this city; once by myself, on a patient who survived the operation seventeen days, and once, six weeks ago, by my friend Dr. Gordon, on a case which is at the present moment going on favourably; and that in neither the one or the other of these cases were the terrible expectations of Mr. Alexander Shaw realized.

The wounds, of which one was about five, the other a little less than four inches long, suppurated, and granulated healthily, and were not sources of any considerable discomfort to the patients.

Meningitis, going on to suppuration, was not a cause of death in any of the recorded cases in which *post mortem* examinations were

made after the operation. In Oldknows' case, and indeed in Holscher's, the dura mater was thickened, and the pia mater of the cord vascular; in Meyer's case the spinal marrow was softened; as however, in my own limited experience, I have seen three patients, not operated on, carried off in great suffering by suppurative meningitis, the whole cord being bathed in pus; I must rather incline to the opinion that trephining tends more to prevent meningitis than to cause it.

Those surgeons who are opposed to the operation of trephining the spine are of course anxious to show that cases of fracture of the spine are not in all instances fatal; and it is obvious that before any surgeon would be willing to incur the weighty responsibility of recommending this operation to his patient, the very first thing he will do will be to inquire what may be the probable chances for life in cases left without operative interference. We have not, unfortunately, anything like accurate statistical information on the subject. It is well known, however, that recovery under such circumstances is extremely rare: as Jones observes, in common parlance, when it is said that a man has broken his back it is generally regarded as synonymous with his having met his death. Mr. Shaw mentions two cases of recovery: the first is that of a patient who recovered (with paralysis remaining) from fracture in the dorsal region, and who was seen by him in perfect health otherwise, more than twelve years after the accident. The second is that of a man who had fracture and paralysis in his youth, who shortly afterwards recovered sensation, and the power of motion in his limbs, so as to be able to follow an active occupation, who, after a period of seventeen years, had a return of the paraplegia, and lingering for five years in that condition, died under the observation of Mr. Shaw, who had the opportunity of examining the body.

He narrates also a very remarkable case communicated to him by Mr. Page, of Carlisle, in which a gentleman, twenty-six years of age, survived, for nearly fifteen months, an injury of the fifth (or fourth?) cervical vertebra, the body of which was broken, almost severing the spinal marrow.

Cases such as that detailed by Mr. Hilton, in which a patient with fracture of the cervical vertebræ, accompanied by complete paralysis of the arms, legs, bladder, and rectum survived for fourteen years; or Mr. Phillips'[a] well-known case of fracture of the

[a] Medico-Chirurgical Transactions. Vol. xx., p. 78.

atlas and axis, are looked upon with an amount of astonishment which is in itself the best proof of their scarcity.

Sir Astley Cooper alludes to two cases in which union by ossific matter had taken place; in one of these the patient died in twelve months, wanting nine days, after the injury, owing to a sore on the tuberosity of the ischium and disease of the bone. The bodies of the first and second lumbar vertebræ were found to have been fractured and united by ossific matter. In the second the history of the case was not known, but the fracture was united by ossific matter.

In the museum of the Royal College of Surgeons of Edinburgh there is a fracture of the vertebræ, in the dorsal region, which united by callus, although the patient only survived the accident about two months.

Mr. South has notes of a case in which union of a vertebral fracture, accompanied by perfect palsy of the lower limbs and bladder, ensued, and the patient continued improving for ten years; and he has in his possession two preparations of cases in which the patients lived long enough to have callus thrown out at the seat of fracture. Most practical surgeons will agree with him and Jæger that "many cases of pretended union of fractured vertebræ are very suspicious, and may have been only mere bruising, stretching of the ligaments, or concussion of the spinal marrow." I have heard, indeed, of some wonderful cases of recoveries from fractures of the spine, but one is forced in these cases to doubt the correctness of the diagnosis on reflecting upon how few are the specimens in our museums of cases like that mentioned by Mr. Shaw, where years after the injury pathology revealed that fracture had actually existed.

I would remind my readers also that in some cases of anchylosis of the vertebræ, resulting from disease, the condition is not unlikely to be confounded with anchylosis arising from the effects of injury. Chronic rheumatic arthritis, which has little, if any, tendency to produce anchylosis elsewhere, has a tendency to do so in the spinal column, and that too in a manner which, without careful examination, might be mistaken for anchylosis, the result of fracture. A preparation indeed of this kind, in which anchylosis existed between the second and third cervical vertebræ, was accompanied by appearances so remarkable as to induce some of those who examined it to think that injury, with fracture, had been the cause of it, yet from the history of the case it was certain that it was due to nothing of the sort, but resulted from rheumatic disease, with an unusual

congenital formation. It is therefore possible that some specimens, of the history of which nothing is known, may be supposed to be united fractures or displacements where anchylosis has followed, which really are not so.

It is certain, however, that cases of recovery from fractures of the spine, with symptoms of compression of the cord, are of extreme rarity; one per cent. is greater than Sir Astley Cooper met with in his experience. The proportion of cases in which, after operation, life was preserved shows a per-centage which contrasts very favourably with this.

But it may be said if the correctness of diagnosis is to be doubted in cases which live without operation, is it not equally doubt ul in those where patients do not die after the operation. This is of course true, and this is precisely the reason that in attempting to form a fair judgment of the merits of the operation in question I conceive that in one point of view the unsuccessful cases afford stronger testimony in favour of it than those reported as successful. In the one set of cases we remain in the dark as to the precise nature of injury; in the bulk of the unsuccessful cases, where *post mortem* examination left no doubt as to the nature of the injury, enough of improvement has followed to prove incontestably that the theoretic arguments urged against the operation are not well founded. Those cases, however, in which patients have survived the operation have a great value even if it be supposed that in no one of them was a fracture really existing; they show that there is nothing in the operation of trephining the spine which, of necessity, must cause death; they show, if nothing else, at least that the "inevitably fatal" predictions of Sir Charles Bell, and the gloomy forebodings of Mr. Alexander Shaw are equally unverified by facts.

It must not be forgotten that some of the most eminent British surgeons who have ever lived have spoken favourably of the operation of trephining the spine. "Let the young surgeon's mind," says Mr. Shaw, "be fortified against the influence of the authority in favour of the operation given by certain great men in the profession." I would indeed, in the consideration of such a question, repeat let the surgeon's mind be fortified against the influence of authority; let him rather consider the facts of the case, and what is said, than who it is that says it. I refer therefore to the observations of Abernethy, Benjamin Bell, Cline, Sir Astley Cooper, South, &c., not in order to call in the influence of the authority of their names, but to show that surgeons of vast experience regarded

fracture of the spine as an injury of so fatal a character that they judged favourably of an operation which seemed to them to give a chance of saving even one life out of many.

Abernethy, with characteristic caution, says of the operation, "I think that it is a proposition too hardy to be acquiesced in; but I see in it nothing but what is rational—it is the only mode that occurs to one's mind."

Benjamin Bell speaks more strongly:—"Wherever," he says, "the spinal marrow appears to be compressed, and where there is reason to think that the compression is produced by the depression of a portion of bone, as we know from experience that every such case will terminate fatally, if the cause of compression be not removed, it would surely be better to endeavour to raise it, than leave the patient to certain misery and death. By laying the injured parts freely open we may be enabled to raise that portion of bone by which the compression is produced; while, in such circumstances, it cannot add to the hazard of the patient, even allowing the attempt to prove abortive. In a case where symptoms of paralysis were induced by a musket-bullet lodged in the substance of one of the vertebræ, a complete recovery was obtained by extracting the bullet. A portion of depressed bone might often be removed with equal ease and safety; and there is reason to suppose that similar effects would often result from it."

Sir Astley Cooper, as is well known, thought well of the operation. In his lectures he says, that "the proposal is laudable, and the operation not severe, nor does it increase the danger of the patient; time and experience can alone determine its value. If we could save one life in a hundred by it we should deserve well of mankind, and if any good does ultimately result from it, Mr. Henry Cline[a]

[a] In this Sir Astley Cooper was in error; the operation was performed by Louis before Henry Cline's time; and it was more than a hundred years ago, at least, recommended, if not performed, by Heister. He says:—"Wenn aber die Fractur so gross und hefftig, dass dadurch das Rückenmark selbst gequetschet und verletzt ist, so folget gemeiniglich bald der Tod. Dennoch, weil auch solche Patienten nicht zu verlassen, soll man wohl untersuchen, wie die Fractur beschaffen; und wenn man etwa spühret, dass Trümmer von den wirbelbeinen gantz abgebrochen, selbige herausnehmen: weswegen man aber offt eine Incision machen muss. Solten Stücke von den gebrochenen Beinen das Rückmark drucken muss man solche entweder mit den Fingern oder mit dienlichen Elevatoriis oder Zangen trachten in die Höhe zu heben, oder wo sie los sind, gantz herauszunehmen. Nachdem muss man suchen die wunde von aller Unreinigkeit wohl zu reinigen."—u.s.w. (Laurentii Heister's Chirurgie, &c. Neue Auflage. 1747, 4, s. 184.)

In Dr. James's Medical Dictionary, published more than a century ago, we find

has the merit of proposing it. *Palmam qui meruit ferat.*" That this was not an expression of opinion formed without due deliberation, and uttered, perhaps, hastily in an oral discourse, is clear from the same statement being repeated in his work on Fractures and Dislocations. When speaking of injuries of the spine, he says, "Mr. Henry Cline was the only person who took a scientific view of this accident. He considers it to be similar to fracture, with depression of the cranium, and to require that the pressure should be removed; and, as the cases had proved so *uniformly fatal*, he thought himself justified in stepping out of the usual course, with the hope of preserving life." . . . . "If the operation saves only a life in one hundred, it is more than I have yet seen accomplished by surgery." But more than this, Sir Astley gave practical proof of his approval of the proposal by operating in a case in which there was supposed to be fracture with displacement; it turned out, however, to be merely a fracture of the spinous process, of a dorsal vertebra at its root, and therefore the arch was not meddled with.

Mr. South, of St. Thomas's Hospital, has advocated this operation, which he himself performed on one occasion, although without success. His excellent observations on the subject, in the notes to his translation of Chelius' Surgery, are well known. As regards the violence supposed by some to be necessary in the performance of the operation, and with reference to the patient's suffering, he says:—"I can only say that in Tyrrell's operation which I witnessed, and in that which I performed, no violence was either required or used; in neither did the patient suffer much, nor was his condition rendered worse, but in Tyrrell's case immediately improved." I may add, that in my patient's case the operation was not attended with much suffering; although during a great part of the time he was not thoroughly under the influence of chloroform, but was speaking to those around; the bystanders were surprised at how little he seemed to suffer.

Mr. South discusses the question of operation with great clearness

almost the same; his words are:—"If the spinal marrow is wounded, death follows inevitably; though as it may seem cruel not to attempt the relief of one under these unhappy circumstances, the surgeon should lay the injured part bare by the knife, and elevate the fragments which press upon the medulla, in a proper manner; or when they are quite loose, extract them. Then let him cleanse the wound thoroughly, and apply balsamic medicines, using the napkin and scapulary. He must continue this till the wound is healed or the patient dies."

and candour; and, although led to form a judgment in its favour, he admits what I agree with him in thinking to be the most reasonable objection made to it, as well as by far the weightiest, which can at present be urged against it; this is simply the difficulty of accurate diagnosis in cases of supposed fracture of the spine. But the argument derived from difficulty of diagnosis is common to a great number of obscure surgical affections, without its being considered sufficient to give rise to an established rule, that on that account operative interference is not to be contemplated. If I understand rightly the views of those who are in favour of the operation, they ask nothing more than this: that it shall be acknowledged that in certain cases this operation is justifiable; that it shall not be taught in our schools, that as a matter of course, in all cases of fractured spine operation is worse than useless, and not to be thought of; but that the proceeding of trephining the spine shall be permitted to take its place among the legitimate operations of surgery, as one which although formidable, and in most cases not likely to prove successful, nevertheless holds out by far a better prospect of saving life than the system of doing nothing.

It is in the highest degree interesting in scientific questions to find persons starting from different points, or at least pursuing different routes, yet arriving at the same conclusion. Such is in itself strong testimony in favour of the justness of the conclusion arrived at. When we find, therefore, such a man as Dr. Brown-Séquard, who views the question now under consideration more as a physiologist than as a surgeon, and when we find that as a physiologist he is led strenuously to advocate the operation of trephining the spine, we have in this fact additional evidence in its favour. For my own part, I consider Dr. Brown-Séquard's reasoning upon this subject to be unanswered and unanswerable. I say unanswerable, because I regard Mr Shaw's criticism as quite unsatisfactory. Dr. Brown-Séquard, indeed, goes too far when he says, that "these various operations ought to be employed in *almost all* cases of fracture of the spine;" but when Mr. Alexander Shaw accuses him of "omitting to inform us how the patient—thus all but completely decollated, the muscles of the neck extensively cut, the vertebræ deprived of their posterior arches, their bodies broken across, and the ligaments between them ruptured—should be able to hold up his head, or prevent its rolling to and fro, so as to crush the cord supposed, before the operation, to be sound," he speaks without reflecting; he forgets that in Jones' case, at least, this

"complete decollation" did not occur, and that in truth, the ligaments connecting the bodies, transverse and articulating processes, are hardly ever so entirely lacerated as to permit of its occurrence. Even when the body of a vertebra is artificially severed, the parts are still so firmly held together that the removal of the posterior arch does not materially weaken the union caused by the ligamentous structures. This I can state from having made the trial on the dead subject.

It is quite obvious, however, that in case of operation in the cervical region, a suitable appliance to keep the head and spinal column immovable would be indispensably necessary. Jones speaks of the necessity for some such apparatus in cases like his, and he thinks that the application of some well constructed appliance might have obviated the fatal issue in that instance.

It is needless for me to recapitulate Dr. Brown-Séquard's[a] arguments; those interested in the subject will peruse them for themselves. But there is one topic of which he speaks of such vast practical importance that I must be allowed to dwell upon it at some length.

The proposition which it is all important to bear in mind in the present discussion, *that death after fracture of the spine usually is due to pressure or some excitation of the spinal marrow, not to partial or complete section of this organ*, will be readily admitted by those who have had leisure to follow the teachings of modern physiology on this subject. They will know that the disturbance of the functions of sensation and motion, are of little consequence in these cases when compared with the disturbance of the vaso-motor functions of the cord. The presiding influence which the spinal cord exercises over the blood vessels, the disturbance of which produces such remarkable effects upon nutrition, secretion, and temperature, is the function, the consideration of which in cases of fractured spine, demands our real attention. Patients do not die because they have lost sensation, or motion in their lower limbs, or half their body. They usually die from the alterations of secretion and nutrition, the principal evidence of which we find in the rapid atrophy, the sloughing and bed sores, the changed condition of the urinary secretion, the inflammation of bladder, ureters, and kidneys, which constitute so terrible a train of symptoms. The experimental physiologist very well knows that morbid excitation of the cord is

[a] Lectures on the Physiology and Pathology of The Central Nervous System. Appendix, p. 245.

the cause of these disturbances, and that a more or less complete section of the cord does not give rise to them, or at least does not cause them to so great a degree, as the prolonged morbid excitation resulting from pressure, or from the driving in of a spicula of bone.

It is therefore a physiological truth which the surgeon should ever have present to his mind, that morbid excitation of the cord resulting from pressure, is the cause of the train of symptoms, which in the cases under consideration, he has the greatest reason to dread. If he can remove the pressure he does all that human skill can do to get rid of the excitation which (unless the injury be high up), step by step gives rise to excoriations, bed sores, sloughing, alkaline urine, inflammation of the bladder, urinary passages, and kidneys. As I have already said, by removing pressure, the surgeon does all that can be done to break the chain of sequences, which when uninterrupted, leads the patient's suffering from fractured spine so inevitably to the grave. But it may be said, the experimental physiologist may have no difficulty in admitting the foregoing proposition; he may know that Henry Cline, and many others, have found that animals recover after section of the cord; he may know that it is not section of the cord but persistent irritation of it from some cause which gives rise to disturbance of the nutritive and secretive functions; but what proof have we that such is the case in man. There is indeed proof enough to satisfy the most sceptical. I have myself seen too cases of wound of the spinal cord in man, made by cutting instruments—one in the London Hospital, another at the National Hospital for the Paralyzed and the Epileptic, London—in neither, although paralysis of motion and sensation existed corresponding to the amount of injury done to the medulla, was there sloughing, alkaline urine, inflammation of the bladder, &c. But in proof of the foregoing proposition I shall content myself by referring to two cases:—

"Case III.[a]—A drummer of the National Guard of Paris had a quarrel with one of his comrades, who was drunk, and who, not being able to reach him, flung his sabre at him from a considerable distance, just as he was retreating, and had his back towards his antagonist. The point of the instrument reached the upper and back part of the neck. The wounded man at once found his legs give way under him and fell. He was conveyed next day to the

[a] Boyer, "Traité des Maladies Chirurgicales," Tom. vii. p. 9.

hospital of La Charité. The wound, the borders of which were a little contused, was about two inches in extent; it was situated immediately below the occiput, in the upper and back part of the neck and on the right side. The finger could neither discover the depth of it nor penetrate to the vertebral column. The right arm had lost its power of movement, but preserved its sensibility. The right leg seemed somewhat weakened, but was normally sensitive. There was slight difficulty in breathing; pulse frequent, strong and full. He was bled, &c. On the fourth day the weakness of the right leg had entirely disappeared; the patient was able to make some slight motion of extension of the forearm, but could not spontaneously make an attempt at restoring it to the state of flexion.

"On the thirteenth day the patient had recovered his strength and his appetite—he got up and walked; but the paralysis of the upper extremity was the same. When playing tricks with a wardsman who pinched him, he perceived that the left side of the body was insensible. He told me of it the next day, and I observed the following phenomena:—The left lower limb and the left side of the trunk were of the ordinary size, and possessed their power of movement and their usual agility; but the skin of all these parts might be pinched, pricked, and even cut without the patient feeling it or giving the least sign of pain; pins were stuck in to the depth of three or four lines, and when his head was turned away the man did not perceive it. However, extensive contact, as the application of the hand laid on flat and moved over the surface, gave rise to a sort of sensation, but very slight and obscure. This insensibility existed in the entire extent of the left foot, leg, and thigh; it was equally complete over the left side of the abdomen, but it ceased abruptly in front and behind, at the middle line, with this remarkable peculiarity, that in this locality, if the patient was pinched on the left side, he asserted that he felt a sensation, though feebly, at a corresponding point on the right side. A similar demarcation between the right and left side extended to the skin of the penis and scrotum. The insensibility was complete up to the left side of the base of the thorax; but at a little higher level an obscure sensation began to be perceived, which became more distinct higher up, so that at the level of the fourth rib the skin had natural sensibility. The left limb was in a perfectly natural condition. Twenty days after his accident this man left the hospital cured of the wound in his neck, and having no pain or uneasiness in this part; but the right arm, forearm, and hand were almost completely

paralysed, and the left side of the body, excepting the upper limb, were in a state of insensibility which has just been described. Some months after he revisited the hospital; his condition was little changed. The situation of the wound, and the symptoms with which it was accompanied, induced one to believe that the spinal marrow had been engaged.

The second case was one of the most dreadful that I have ever had the opportunity of witnessing:—

"John Neal,[a] a tinplate-worker, was admitted into the Whitworth Hospital, January 31, 1861. He was a thin, delicate-looking man, and presented palpable signs of having suffered severely from syphilis; his skin bore the marks of phagedenic ulceration; the right eye had been the seat of severe inflammation, the pupil was contracted and irregular, and vision impaired; and the left testis enlarged and indurated.

"On admission, he complained of a sense of constriction round his waist, of a difficulty in standing upright, and of general debility. A week afterwards he began to experience difficulty in passing water, and the urine became alkaline and offensive. On the 11th of February he had a rigor, and walked with a staggering gait; and on the 15th, paraplegia was almost completely established, and the sensibility of the lower extremities was somewhat impaired.

"On the 20th, constitutional symptoms of an alarming character were observed, resembling those which follow the infliction of a severe injury There was cold perspiration over the surface, the pulse was weak and intermittent, and there was frequent vomiting.

"From the 1st of March to the 15th, whilst terrible bed sores were forming, a change was observed in the phenomena of the paraplegia. Some degree of voluntary power was regained in the left foot and leg; the former could be flexed and extended, and the latter drawn up in bed to a certain extent; no such improvement, however, took place in the other limb; but in this the reflex muscular movements regained much of their normal activity, whilst there was a partial recovery of sensibility in both limbs. Whilst, therefore, sensibility was equally impaired in both lower limbs, these extremities were contrasted in the degree of voluntary and of reflected muscular motion which they possessed.

"On the 15th of March, the sloughing had reached a shocking

[a] Case by Dr. M'Dowel, Proceedings of Pathological Society, April 6th, 1861, p. 173.

extent; branches of the gluteal artery poured out florid blood at each dressing, and a large portion of the left sciatic nerve was completely exposed. The sufferings of the patient were now very severe; yet in spite of the profuse discharge, the bleedings and consequent hectic, he lingered until the 2nd of April, when he died, emaciated to the last degree. The ravages made by the sloughs were such, that not only was the sacrum exposed and partly softened, but the small rotator muscles and the gluteal nerve on one side, and on the other even the capsular ligament of the hip-joint, were plainly exposed to view.

"Sixteen hours after death the spinal cord was carefully removed. In the centre of its dorsal portion, but extending considerably to the right of the middle line, a tumour of firm consistence and yellow colour was exposed, when a vertical section of the cord was made. It was of the size of a large pea, of a globular form, and smooth on its surface and section; the cord above and below it was softened, and in parts abnormally vascular. In the centre of one area of vascularity a small yellowish spot was observed. There was no trace of tubercles either in the brain, lungs, or liver. The bladder was contracted and inflamed."

In one of those cases we see that a partial section of the spinal cord did not occasion to any formidable extent, disturbance of the functions of secretion and nutrition; while in the second, a small tumour producing morbid excitation of the medulla, just as the pressure of bone does, was followed by the most terrible ravages.

In the latter case, had it been possible to know the true state of things, it would have rendered the patient's condition less dreadful to have cut the cord clean across above the disease; and, I believe that in many cases of fractured spine, it would be better for the patient to have the medulla cut across above the injury than to have it left subject to the pressure and irritation of the displaced bone. Let me not be understood to propose such a proceeding; I only say, that even that would be better than doing nothing to remove the pressure, which is the first origin and main spring of what finally kills the sufferer in the great majority of cases.

We learn from Mr. South, that before undertaking the operation of cutting away that part of the arch of the vertebra which presses the cord, Henry Cline proposed to himself four questions. It is interesting to compare the answers given by Cline to these questions, with those which, profiting by the experience

and knowledge gained since then, may now be given to them. Cline says:—1st. "Will the patient die of the operation?—Probably he will, if the injury be severe." This question, I presume, means will the operation of itself prove immediately fatal to the patient. Cline probably shared the belief, still indeed held by some, that the exposure of the cord or its membranes, or by chance, the letting out of the cerebro-spinal fluid, was excessively dangerous, and likely to cause convulsions and sudden death; he may also, as South suggests, have supposed that exposure of the cord would hasten inflammation and death. We now know that none of the recorded cases have died suddenly; that in no case can it be said that the operation materially hastened the issue to be expected if it had not been performed, and that it has not been followed by meningitis or myelitis, so as to hasten death. In short, no alarming hemorrhage takes place, and in the operation itself, if performed with care, there is no danger.

2nd. "If the cord be much hurt will it recover its functions?—This is unknown; but we do know that if a nerve be divided, it will unite, and the greater part of the spinal cord may be divided in a brute, and yet the animal recover, in proof of which is detailed an experiment which was performed on a bitch. A cut was made at the back of the neck through the muscles, and the yellow substance between the last cervical and first dorsal vertebra having been divided, the handel of a scalpel was pressed firmly down on the cord, and the result was, that the parts behind the division were completely palsied, excepting that the tail continued still capable of a very slight motion, proving that a small portion of the cord was still undivided. With very great care and attention the animal recovered after some months, and resumed all her paces as usual, with the exception of a peculiar movement of her hind limbs in galloping. She was afterwards destroyed, and the spinal cord being examined, was found to have united. This then seemed to be sufficient proof that a spinal cord which had been considerably injured, if not all but entirely torn through, might be re-united and recover its functions." It is quite certain that the spinal cord may be injured, and afterwards recover its functions. Experiments like Cline's have proved this over and over again. In repeating Dr. Brown-Séquard's well-known experiments on the spinal cord, I have often, more or less, completely cut the cord, and have often seen the animals operated upon almost entirely recover from the paralysis resulting from the incision, and rarely known them to die from a section of a lateral half of the cord.

3rd.—"After the removal of the arch of the vertebra, will the spine be sufficiently strong to support the body?—Probably it will for ordinary purposes, though the patient may not be able to lift heavy weights." Louis's case recovered so as to be able to walk with a stick; none of the other cases regained so much power. The result as regards the power of walking is not mentioned in the short notice of Edwards' case, given in the *British and Foreign Medico-Chirurgical Review*, but through the kindness of my former pupil, Mr. Phillips, of Cyner, in South Wales, I have learned the following particulars regarding this case:—The patient, whose name was Griffiths, was operated upon a considerable time after receiving the injury; he lived for fifteen years after the operation, but never was able to walk; he regained complete control over the bowels and bladder, and became the father of children.

4th.—"Will a patient recover from a compound fracture of the spine, which by the performance of the operation it becomes?—The nearer a fracture is to the source of the circulation, and the less compact, and consequently more vascular the bone is, the greater is the probability of recovery. Both these advantages are present in the spine, and therefore favour the successful issue of the case." Experience has answered this question in the affirmative.

There are of course many questions which arise in connexion with this subject, as regards, for instance, the symptoms indicating cases favourable for the operation, or the reverse, and methods of performing the operation in the different regions of the spine, &c.; but such questions are quite secondary to the great one, as to whether the operation of trephining the spine is justifiable or not. It is to the discussion of this that I have confined myself in this communication, and I cannot help believing that the case I have had to report, although unsuccessful, goes a long way in support of the view which I adopt. I sincerely hope that the perusal of its details will induce surgeons to reconsider a subject which must be admitted to be one of the most important within the domain of surgery.

I have made mention in the foregoing pages of a case in which my friend, Dr. Gordon, has performed the operation of trephining the spine. The full details of the case will be, no doubt, laid before the profession as soon as time makes clear the result. As the case is one, in my mind, of extreme importance, and as the patient's progress for so far is very gratifying to me, inasmuch as Dr. Gordon was, in a great measure, led to undertake the operation

from a careful consideration of the details of Collins' case, and inspection of the injured parts; I have obtained his permission to state the following:—

The operation was performed on June 3rd, two months after the accident, and in the same region as in my case. I now write on July 22nd. Just seven weeks, therefore, have elapsed since the operation. Of the wound not so much as half-an-inch in length remains unhealed; a minute portion of bone (weighing nearly two grains) came away; no abscess formed; in fact Mr. Shaw's appalling predictions have not been realized. Motion is in an appreciable degree restored in the lower limbs; sensation has improved; a bed sore which had formed on the back, not larger than half-a-crown, is well; the urine which before the operation was alkaline, containing blood, and copious muco-purulent deposit, and very offensive, is now almost natural in colour, and has been for the last month persistently acid; power over the bladder is restored. The patient's appearance is much improved, and his appetite is good.

Mr. Harman, one of the resident clinical clerks in the Whitworth Hospital, has copied from his note book the following, showing the condition of the urine and bladder succeeding the operation:—

Fourth day after operation.—Urine became acid.

Fifth day after operation.—Urine again alkaline, scalds him in passing.

Eleventh day.—Had a motion from the bowels, of the passage of which he was perfectly conscious, but unable to control. Urine since last date alkaline, drawn off with catheter.

Fourteenth day.—Urine when drawn off, acid, and quite free from blood.

Sixteenth day.—Urine slightly alkaline, and continued neutral for three days when it became acid, and has remained so since (now a month).

Twenty-sixth day.—Can eject the urine from the bladder; and since the beginning of July the power over the bladder is such as to force the water strongly against the side of the vessel in a full stream, and on one occasion to a distance of between two or three feet; urine persistently acid.

The catheter is no longer necessary.

Of course I need not say that as regards the life of this patient, the improvement in the urine, bladder, bed sore, and, I may add, the state of the penis, is by far more important than the return of sensation or any return of motor power.

APPENDIX.

Reference to the Cases in which Operation has been Performed in order to Remove Bone causing Pressure on the Spinal Cord.

No	Name of the Surgeon who operated on the case	Reference to where the case is mentioned or detailed
1	Louis, - -	Archives Génerales de Médecine, 1836, p. 397
2	Henry Cline, -	New England Journal of Med. and Surg., Vol. IV., No. 1
3	Wickham, (Winchester), - -	Sir A. Cooper's Lectures, edited by F. Tyrrell, Vol. II., p. 20
4	Attenburrow, -	*Ibid*, p. 20
5	Oldknow, - -	Sir A. Cooper's Treatise on Dislocations and Fractures, new edition, by Bransby B. Cooper, 1842-8, p. 560, Case 352
6	F. Tyrrell, - -	Lectures of Sir A. Cooper, &c., by Tyrrell, Vol. II., p. 11
7	J. Rhea Barton, -	Treatise on Dislocations and Fractures of the Joints, American edition, by J. D. Godman, p. 461; and Malgaigne Fractures, translated by Packard, p. 343
8	Tyrrell, - -	Lancet, Vol. XI., 1827, p. 625
9	Alban G. Smith,	North American Medical and Surgical Journal, Vol. VIII., 1829, p. 94
10	Holscher, - -	Hannoversche Annalen, f., d. ges., Heilk. Bd. 4, 1839, p. 330
11	South, - -	Chelius' System of Surgery, translated by South, Vol. I., p. 540
12	D. L. Rogers, -	Amer. Jour. of the Med. Sc., Vol. XVI., 1835, p. 91
13	Edwards, - -	British and Foreign Medical Review, Vol. VI., 1838, p. 162
14	S. Laugier, -	Bulletin Chirurgical, T. I., p. 401; and Laugier des Lesions Traumatiques de la Moelle Epinière Thèse de Concours, 1848, p. 133, Obs. 52
15	Potter, - -	New York Journal of Med. and the Collateral Sciences, Vol. IV., 1845, March

No.	Name of the Surgeon who operated on the case	Reference to where the case is mentioned or detailed
16	J. B. Walker, -	A Descriptive Catalogue of the Anatomical Museum of the Boston Society for Medical Improvement, 1847-8, p. 31, No. 141
17	A. Mayer, - -	Walther's and Ammon's Journal der Chirurgie, Bd. 38. 1848, s. 178. Abildung, Taf. 1, Fig. 1-5
18	G. C. Blackman,	Blackman's edit. of Velpeau's Surgery, Vol. II., p. 392; and J. C. Hutchison in Amer. Medical Times, 1861, July 13, p. 21
19	G. C. Blackman,	*Ibid*
20	G. M. Jones, -	Medical Times and Gazette, 1856, Vol II., p. 86
21	J. C. Hutchison,	Transactions of the New York State Medical Society, 1861; and Amer. Medic. Times, 1861, July 13, p. 19
22	Stephen Smith, -	Ch. Phelps in New York Journal of Medicine, Vol. VI., 1859, p. 87
23	Blair, - -	Ballingal's Military Surgery, 4th edit., 1852, p. 298; and Essays of Dr. Munro (Secundus)
24	Potter, - -	American Medical Times, January, 1863, p. 17. Case twice operated upon
25	Potter, - -	American Medical Times, January, 1863
26	Heine, - -	Velpeau's Surgery, 1st American edit.

Art. VII.—*Two Casee of Penetrating Wound of the Chest.* By J. H. Wharton, Surgeon to the Meath and Cork-street Hospitals.

Having, in the course of the last session, brought under the notice of the Pathological Society a brief account of a case of penetrating wound of the chest, and exhibited a specimen which displayed the cause of death, I am induced, through the kindness

of Mr. Hamilton, Surgeon to the Richmond Hospital, who furnished me with the details of the case following, to place it, as well as my own, on record. The fact that it occurred several years ago, when Mr. Hamilton filled the office of Resident Pupil at the Meath Hospital, does not, as I think, militate against its interest, but, perhaps, rather increases it.

"James Quin, aged thirty-five, of a strong habit of body, generally temperate, healthy, was admitted into the Meath Hospital on September 24th, 1832, at 8 o'clock, p.m., in the following state.—He had a wound about an inch in length between the cartilages of the fourth and fifth ribs, on the right side, transverse, and communicating with the cavity of the chest, and round the lips of which there was slight emphysema. During each expiration a strong current of air rushed through the wound. Surface of the body cold, particularly the lower extremities; countenance dejected and pale; pulse scarcely to be felt; respiration short and hurried, thirty-four in the minute; great inclination to doze, though very restless; constantly calling for water from the great thirst; lay as easily at one side as at the other; stated that at 7 o'clock that evening whilst endeavouring to protect a woman from her husband, he was stabbed by him with a case knife (he did not examine the knife afterwards), and that he lost a considerable quantity of blood. The lips of the wound were brought together by adhesive plaster; he was put to bed, given a warm drink, and warmth was applied to the feet. A few hours after admission a stethoscopic examination was made, when it was found that respiration was audible over the whole chest; sound on percussion was dull, only over the angle of the right scapula. 1 o'clock, a.m.—Lies with greater ease on affected side, in which he feels a very sharp pain; surface of the body becoming warm; and the pulse is evidently more full, but still quick. 3 o'clock.—The pain and difficulty of breathing being increased, the pulse though still low was yet somewhat fuller, it was thought advisable to take away some blood, which was accordingly done to the extent of six ounces, with great relief, and he shortly afterwards broke out into a profuse perspiration over his face and chest. 9 o'clock.—On percussion anteriorly the chest is quite clear as far as the sixth rib, below which there is dulness. From the axilla downwards laterally, and also at the postero-inferior portion, over the anterior face of the lung, and all round the wound, respiration audible, and only mixed with an occasional sonoro-sibilant râle. On the side and postero-inferior portion it is wanting. No

bronchial respiration nor broncophony; no emphysematous crackling about the wound to-day as was observed last night. Pulse 130, occasionally intermitting. Ordered five grains of calomel and a quarter of a grain of powdered opium in a pill every third hour, and to be bled to sixteen ounces. He was bled; but after a few ounces were drawn away he felt faint, so that the vein was instantly closed. Pulse 120. 3 o'clock.—Pulse 122, full and bounding; skin hot; tongue white; face anxious, flushed, and with a yellow tinge; great oppression; breathing laborious and abdominal. The posterior half of the chest sounds dull, and respiration inaudible; no cough or expectoration; unable to rise in bed. On consultation it was determined to bleed him and continue the pills, and a few leeches after venesection if necessary. Venesection to near thirty ounces was performed before the pulse became softer. The wound was then tied up, after which he felt faint. 8 o'clock, evening.—Much easier; pulse 120, soft; ordered forty leeches to the side, and to continue the pills.

26th.—Countenance looks well; breathing much easier; passed a good night; has taken six pills; bowels confined; tongue white; pulse 120; no cough; less pain in side; on percussing the right side posteriorly and inferiorly it sounds dull; respiration scarcely audible anteriorly, and inferiorly dull; but respiration more audible, though feeble. Ordered thirty leeches to the side, to continue the pills, and to have an enema at bedtime if necessary.

27th.—Going on well; breathing easier; the mouth is now affected by the mercury; the pulse full, but less frequent, 105; tongue covered thickly with white fur; some cough. On examining the chest anteriorly, which alone was done, there is still dulness of the right side, below right nipple, with very feeble respiration and distressing sibilant râles round the wound, which is now open and discharging a bloody spuma. In consequence of this, fresh adhesive straps with lint were applied; much pain in the abdomen, and frequent purging affected him during the night. Last night forty more leeches were applied, the pills stopped, and a draught ordered, containing castor oil, one ounce, and tincture of the acetate of opium, twenty-five drops. 2 o'clock.—Purging and griping continued all day, and are still present; pulse full; lint wet with blood.

28th.—Still some purging, but slighter; has oppression of the affected side, with a sense of weight, and a feeling sometimes of heat, at others of cold; no pain, except a sting in the wound in

turning in the bed; respiration interrupted; and wheezing; no cough; lies most easily on the back; if he tries to lie on either side he feels for a few minutes pain in the wound; the lint is wet with blood, and some oozing from the sides; the chest, anteriorly, looks fuller than the opposite side; tongue loaded; pulse 105, full and strong; feels weak; dulness extends higher up, particularly along the side; posteriorly, respiration more feeble; the affected side is one inch larger than the other by measurement. 3 o'clock.—On consultation it was deemed advisable to enlarge the wound, with a view of giving exit to supposed fluid in the cavity of the chest; no blood or fluid escaped through the wound, nor was any obtained by the use of a syringe or introduction of a bougie far into the wound, or by position. During this operation the patient experienced inconceivable distress; his countenance became anxious and pale; much palpitation; respiration 44; pulse weak. This distress was much abated by the closure of the wound.

29th.—An hour after the operation felt weak, and complained of pain in the lower and back part of the chest, which he had not done before; suffered greatly from anxiety; passed a bad night; at present complains of pains in the side and tenderness; respiration still laborious, and not so evident at the affected side, which appears fuller; dulness on percussion, and feebleness of respiration, if anything, increased anteriorly; not examined posteriorly; respiration in opposite side puerile; some sibilant râles; pulse 126, soft. Last night at 8 o'clock, thirty leeches and the cupping glasses were applied to the lower part of the affected side. Ordered to take two grains of calomel and half a grain of opium three times before morning, provided no diarrhea supervened; in which case they were to be discontinued, and an enema of starch and opium administered.

30th.—Passed a very restless night, constantly changing his position, and getting no relief in any; felt great pain in lower and posterior part of right side; constantly covered with perspiration; respiration short, hurried, difficult, and loud, forty in a minute; pulse 114, and weak; manifestly losing strength, and greatly dejected. On consultation at half-past eleven o'clock, it was determined to make an opening in the lower and posterior part of the chest, which was accordingly done; and when the cavity was penetrated an inordinate quantity of bloody fluid rushed through the wound. This was as dark as venous blood, and did not coagulate on rest; it was forced out during expiration; the fluid not coming

so readily after a little, the tube of a stomach pump was introduced, and a quantity of fluid drawn off by the aid of an exhausting syringe; the patient experienced a good deal of anxiety and distress during the operation; but, though extremely weak, expressed himself relieved and breathed more freely; respiration feeble over the anterior surface of the thorax, accompanied with a moist crepitus superiorly, and an occasional sibilant râle; at the inferior fifth anteriorly, the respiration feeble, and accompanied with a rather dry crepitus, seemingly distant from the surface; percussion elicited a clearer sound; in the upper part of the axilla respiration was loud; inferiorly a sonorous râle, with a sound of two rough surfaces rubbing together, were heard; corresponding to this region the integuments were particularly tender. Half-past 9 o'clock, p.m.—Patient is asleep, breathing quick and short, without so much difficulty as before mentioned, and, without noise; is lying on affected side; appearance much improved; forehead and face covered with a tolerably warm perspiration. 10 o'clock.—He awoke from sleep; says he is much refreshed and easier. Ordered twenty-five drops of the acetum opii; to have a small proportion of wine and water during the night; no oozing from the wound.

October 1st.—Slept little during the night from a teasing cough; he perspired much; his thirst quenched; there was immense discharge from the posterior incision, so as to wet several double sheets. This discharge seemed to be serum slightly tinged with blood; he feels much easier, and has no pain, but is weak; pulse 120; very feeble respiration. 3 o'clock.—Tongue furred, dry; respiration is heard over the anterior part of chest with clear sound on percussion; posteriorly the large half of chest sounds dull. Ordered at night calomel, hippo, and opium, and an ordinary cough mixture.

2nd.—Spent both yesterday and last night very uneasily.

3rd.—The symptoms he had last night increased; he became very restless; his pulse very quick, small, irregular, and intermitting; surface of the body covered with a cold clammy sweat; face pallid; lips blue; a dark circle round the eyes, which were suffused, and looked prominent; at 12 o'clock commenced raving, and died at 7 o'clock this morning.

Post mortem examination.—A handle of a scalpel could be passed through the wound; found to be situated between the fourth and fifth ribs, into the cavity of the right side of the chest. The mammary artery was discovered to have been divided, and a small

coagulum filling up both divided extremities. There was a considerable effusion of lymph round the wound, external to the pleura, and the inferior surface of the lung was covered with lymph. The diaphragm was pierced, and the point of the knife had apparently grazed the liver—a spot having been seen corresponding to the wound ecchymosed and lymphy. The lung was not wounded—the knife having passed between the middle and lower lobes. The diaphragmatic pleura was covered with effused lymph, and glued to the lung. There was also considerable interlobular effusion gluing the lobes together. A coagulum was found in the cavity of the pleura, and much serous fluid of a reddish colour. The lungs showed that bronchitis existed, but no parenchymatous inflammation.

The above details are so full, and the symptoms, progress, and termination of the case so similar to that of John Hart, which came under my own observation, that it is only necessary to note and compare their principal features, which I shall most readily effect by means of the following arrangement, and thus save the reader's time.

CASE OF JAMES QUINN.	CASE OF JOHN HART.
Patient's age, 35; habits *generally* temperate; wound 1 inch in length, between the cartilages of the fourth and fifth ribs, on the right side; transverse in direction; *unmistakably known* to communicate with the chest; emphysema present; on expiration, current of air through wound; temperature of the body cold; countenance dejected, pale; pulse scarcely to be felt; respiration short, hurried; thirst constant; decubitus at either side. Local treatment:—Lips of wound were brought together by adhesive plaster; stethoscopic examination, made anteriorly and posteriorly; respiration audible over the whole chest; sound, on percussion, over the angle of the right scapula, dull; sharp pain in affected side; venesection and extensive leeching practised.	Patient's age 22; habits *dissolute*, and habitually intemperate; wound one inch in length, between fifth and sixth ribs, on the left side, the external margin in a line with the nipple, transverse in direction, not *unmistakably known* to communicate with the chest; no emphysema; no current of air through wound on expiration; temperature, appearance of the countenance, state of the pulse, respiration, thirst—similar; decubitus on the back. Local treatment similar—a compress and bandage having been used in addition so as to throw out of action the intercostal muscles; no stethoscopic examination made, except anteriorly, over and above the bandage. In the latter situation, a very clear tympanitic sound was discovered on percussion, as if produced by pneumothorax; respiration, audible over the whole chest anteriorly; sharp pain in cardiac region; no venesection practised, and a single application of a few, six, leeches made use of; no mercury used until the fifth day after admission and receipt of the injury, and then, in consequence of the occurrence of a friction sound over the left
Patient placed under the use of mercury in a few hours after admission to hospital.	

Bleeding from the wound externally on the third day after admission.

On the fourth day evidences of internal hemorrhage were so manifest, particularly from enlargement of the affected side, that the wound in the chest was incised for the purpose of giving exit to the contained fluid. As this operation was not successful an opening was made on the succeeding day into the chest, on the affected side, at its postero-inferior portion, when an inordinate quantity of bloody fluid was removed. Its colour was that of venous, and it had not the property of coagulating; temporary relief ensued; the patient survived the operation till the third day after its performance, and lived nine days after the accident.

anterior portion of the chest, in doses of one grain of calomel and half a grain of opium, at regular intervals; subsequently antispasmodics in cough mixtures were exhibited. Prior to this date Battley's liquor of opium was freely administered, and continued for four days; ice was largely and constantly prescribed, and iced beef-tea and iced brandy and water for diet. No bleeding from the wound externally till the sixth day after admission, as shown by the dressings remaining free from blood stains. No *evidence* of internal hemorrhage till seventh day after admission, when, in consequence of a violent attack of delirium, similar to delirium tremens, the patient rushed out of bed; respiration, which was up to this time *comparatively* tranquil, became hurried, and the dressings were freely stained with blood; dulness by degrees replaced the clearness which had previously existed along the left axiliary region, and anteriorly, rendering it but too evident that that side of the chest was becoming more and more occupied by the presence of a fluid, by means of which the heart was gradually displaced towards the right side. All medical aid having proved abortive, it was determined to perform the operation of paracentesis; forty-eight ounces of fluid, precisely similar to that removed in Quinn's case, were drawn off, but without relief. The patient survived the operation three and a-half hours, and the accident fourteen days.

Post mortem Examination.

CASE OF JAMES QUINN.

Direct communication of external wound with the internal; the right mammary artery was divided.

Coagulum found in the divided extremities of the vessel.

Considerable effusion of lymph round the wound external to the pleura, and on the inferior surface of the lung. The lung

CASE OF JOHN HART.

Indirect communication; the trajet of the wound passing in a direction downwards and inwards, anterior to the cartilages of the sixth and seventh ribs, which were divided near their junction with the sternum; the mammary vessels of the right side being necessarily severed; no trace of coagulum in the vessels, which were empty and flaccid; no lymph round the wound or elsewhere, except on the postero-inferior surface of the sternum, and this deposit was soft, in small quantity,

was not wounded, but much serous fluid of a reddish colour was found in the cavity of the chest, and also a coagulum.

and presented the appearance of a semi-solid pus. The lung was soft, engorged, and collapsed, and neither it nor the pericardium was wounded; a considerable amount of fluid of the same character as that mentioned above, and a small clot, of a dark colour and soft consistence, was found at the bottom of the fluid.

The difficulties attendant upon the management of penetrating wounds of the chest, with accompanying hemorrhage, are so well known to surgeons, that even were sufficient space at my disposal, it would be unnecessary to refer to them, at least upon the present occasion, I shall, therefore, content myself by alluding, and that with the utmost brevity, to two points, which are, I think, worthy of more consideration than they have generally received.

First.—That sufficient importance is not paid to the necessity of maintaining the blood in such a condition as to favour its coagulating property. If this view be at all correct, it will follow that great caution should be exercised in the use of such means as will be likely to diminish to any appreciable extent its healthy proportion of fibrin. Hence I regret that in the management of this case I had recourse to the exhibition of mercury even to a limited extent. Secondly.—That the same remark holds good with reference to the nervous, circulating, and respiratory systems, the *due* control of which constitutes in itself a not unimportant means of checking hemorrhage, and of controlling the tendency to inflammatory action. Accordingly, in the case of John Hart, opium was so administered as to produce a continuously seminarcotic state for four days, during which period the patient was wonderfully free from any symptoms of an untoward nature. I was induced to have recourse to this drug, and to place my chief reliance upon it, from the marked benefit which followed from its exhibition in a case of accidental rupture of the lung, with pneumothorax and hemoptysis, which terminated favourably; and from a consideration of its well known efficiency in certain lesions of the viscera, where the object of the surgeon is to obtain rest of the organ implicated.

Art. VIII.—*Observations and Cases setting forth the Advantage to be derived from the decided use of Caustics in certain Surgical Diseases, more particularly those affecting the Bones and Joints.* By Frederick Kirkpatrick, M.B., Fellow of the Royal College of Surgeons in Ireland, and Surgeon to the Hospital Wards set apart at the North Dublin Union for the Treatment of Surgical Cases.

The history of modern conservative surgery during the last ten years, and the successful results that have been attained by the bold use of the knife in cases of carious bone, or diseased articulation, are so strikingly calculated to excite the admiration of the surgeon, and to stimulate his ambition to follow in the path of operative surgery, that I am diffident of obtaining attention to the mode of treatment which it is my object to advocate in the following pages, more particularly when no claim is laid to original conception, but merely to the more-extended and freer use of a well known remedy. The hospitals of the North and South Dublin Unions afford an unusually large and varied field for the study of disease, and especially of those under consideration; for inasmuch as the general hospitals of the city prefer to admit acute cases that promise a tolerably rapid cure, and that present features favourable for operation, the majority of the chronic cases of disease, consequently, seek relief at the hospitals of the North and South Dublin Unions, where in general they remain the subjects of lengthened observation; and I have thus, during the last twenty-five years in which I have held the post of surgeon to the North Union, had full opportunity of testing the effects of treatment on the various forms of chronic disease, and I have obtained such rapid and excellent curative results from the use of caustics, particularly of the potassa cum calce, that I am induced to lay a brief statement upon the subject before the profession, and to detail some cases where the use of caustic, combined with incision, in the treatment of diseased bones and joints, was attended with remarkable success. For many years I have preferred the practice of opening all chronic abscesses by means of caustic, first making an eschar, and then opening the abscess by a small crucial incision through its centre, and in the case of large abscesses making two or three eschars, and an opening through each. I have also found great advantage from its use in the treatment of scrofulous abscesses, and sinuses in the neck, groin,

and axilla, also in fistulous disease, in anthrax, and in many cases of ulcers, particularly those of a phagedenic character. The principle of combining incision with caustic, which was followed out at the Middlesex Hospital, in the treatment of cancerous diseases, by means of chloride of zinc paste, first suggested the idea to me, and I have constantly since had recourse to the practice, both in the use of the chloride of zinc in cases of cancer, and of other morbid growths, and of the potassa cum calce in diseases of the bones. With the aid of incisions I have thus been enabled to deepen the slough from day to day, and extend its action as deeply aud fully as necessary. I have in this manner treated several cases of disease of the carpal and tarsal bones, and of the smaller articulations of the hands and feet. I have also derived much advantage from the practice in cases of necrosis of old standing in which I used the caustic freely, converting the small fistulous orifices into large funnel-shaped ones, and again and again, at intervals of a few days, carrying its action deeply down to the diseased bone. In some cases the sequestra have been thus divided and easily extracted in portions through the enlarged openings, in others the caustic has induced such a formation of plastic material that the cases have got well without the visible escape of any particles of magnitude. This treatment recommends itself very strongly in a large class of patients, where, from broken down health, and the co-existence of organic disease of internal organs, operative proceedings are so unpromising.

The efforts made by nature to repair disease, so wondrously perfect in wounds and fractures, and, I may say, in nearly all the more acute cases of surgery, are often most ineffectual and tedious when directed to the relief of diseases of the bone. Frequently do we see a patient worn down by hectic, in consequence of a caries, of probably small extent, in the bones of the sternum or scapula, or in one of the pelvic bones; abscess after abscess has formed, small particles of bone have at intervals been washed out of the fistulous openings by the wasting discharge; but after years of suffering the patient is still unrelieved, and the diseased bone is so surrounded and enclosed by a dense, highly-organised, and vascular structure that any surgical proceedings to reach and remove it with the knife is attended with great difficulty, and considerable loss of blood. In all such cases the treatment I propose offers a prospect of speedy relief. In particnlar I advocate its use in caries of the carpus and tarsus where excision has not met with the same measure of success

that its use in the larger joints has been attended with; you can attack each diseased bone separately, boldly boring down to it with the solid stick of caustic, and stimulating its diseased structure to take up a healthy action. In this manner two or three openings can be made, extending deeply down into the diseased bones, without inflicting any serious injury either on the blood vessels or the tendons; the latter can usually be seen when the sloughs have come away, and if the openings are made of sufficient size the bone can also be seen, and examined with a view to the removal of any loose portions.

The number of bones entering into the construction of the wrist and ankle joints, and the communication between their synovial sacs, has always created a reluctance on the part of the surgeon to interfere by operation; and where excision has been attempted extension of disease has been more frequently the result than cure, and in too many cases dangerous consequences have ensued from tetanus, pyemia, or the spread of unhealthy inflammation; consequently the generality of surgeons have been satisfied to trust such cases to the treatment by iodine, rest, and constitutional remedies. The length of time that such a case may last, and the very imperfect hand or foot that remains to the sufferer when consolidation is at last attained, are but too well known to all surgeons. The *Lancet*, March 25th, contains an interesting paper by Mr. Joseph Lyster, of the Glasgow Hospital, on this subject. He, after commenting on the unpromising results of partial operations on the carpus, recommends the removal of the entire carpus, together with the articulating extremities of the radius, ulna, and metacarpal bones, taking away fully two and a half inches from the length of the limb. Mr. Lyster gives several cases of this operation which he performed, that will repay perusal. His operations were formidable ones, and the resulting limbs must have been very considerably altered from their original fair proportions. His paper at all events proves the truth of my proposition, that partial excision of the carpus is not looked upon as a promising operation, and gives me confidence in proposing to the profession the treatment by caustic and incision, aided by well-considered methods for attaining perfect rest to the affected limb. I by no means wish to revive the use of the superficial caustic issues over diseased joints, as I believe it to be a source of hectic and additional waste, and a practice that has been, in consequence, properly abandoned; but used in the manner I advocate, for the destruction of diseased and undermined

integuments, for the opening up of siuuses and fistulous communications with diseased bone, and for the breaking up and stimulating of the carious portion of the bone itself, it is a remedial agent of great potency, and one which I can speak of with the fullest confidence. When brought in contact with carious bone it does not produce much pain, but its action on the skin and soft parts is attended with so much suffering that chloroform should always be administered; however, as the application occupies so short a time, and the resulting pain is so soon removed by cold water, it is not necessary to push chloroform to any extent.

In conclusion, I beg to express my expectation that this mode of treatment will be found equally applicable to the diseases of the larger joints, in all cases where the disease commences in their articulating extremities, and I fully purpose making trial of it whenever I meet cases that present the symptoms of inflammatory action going on either in the great trochanter or in the heads of those bones which form the elbow and knee-joints.

CASES.

Mr. R. S., aged seventeen years, consulted me in April, 1864, for disease affecting all his extremities. I found him in bed, wasted with hectic fever, and his hands and feet enveloped with poultices and oil silk. There were five sinuses leading down to diseased bone in the carpal and metacarpal articulations of the left hand; the right hand was similarly affected, but in a less degree. In the right foot the metatarsal bone and the first phalanx of the second toe were diseased, together with the metatarsal bone of the fourth toe. In the left foot there was necrosis of the metatarsal bone and phalanx of the great toe, and caries of the metatarsal bone of the second toe. Unhealthy sinuses, with undermined skin surrounding them, led down to all the carious bones.

In this case I freely used the caustic treatment, destroying all undermined skin, and carrying a pointed stick of potassa cum calce down to the bone at each spot where caries existed, and making a free direct opening. I stopped poulticing and dressed the sores instead with cotton and salad oil. I saw this gentleman twice a week, and frequently repeated the use of the potassa cum calce to keep the openings free, stimulating the deeper parts by means of astringent lotions and ointments, and with probes coated with nitrate of silver. This patient recovered so rapidly that he was

out fishing in June; in July he was on horseback; and by the end of September all his sores were well excepting the great toe of the left foot, which had been considerably enlarged by necrosis, and was kept irritated by the pressure of his boot; here a small fistula remained during the Winter, but was reduced to the size of a pin hole by April, 1865—one year from the commencement of the treatment. This patient is now the picture of health, but, as a matter of precaution, I have placed an issue under the left clavicle.

E. B., aged twenty-four, of delicate aspect, and labouring under secondary syphilis, consulted me in April, 1861. He had some time previously received a wrench in the wrist of his right hand, which accident had been followed by pain, swelling, and loss of motion. The hand was now pale and puffy; he was quite unable to use the fingers or bend the wrist, and the slightest touch, more particularly of the fore-finger and thumb, gave him exquisite pain. Having made up my mind that the disease was confined to the carpal extremity of the radius, I proposed to bore down upon it at once with the potassa cum calce, and the patient assenting, I made an eschar the size of a shilling over the painful spot, and in two days a small crucial incision in its centre, through which I introduced a pointed piece of potassa cum calce deeply down to the diseased bone. In three weeks, during which time the hand was kept at rest in a splint, the sloughing process was completed, and a clean circular ulcer about the size of a sixpence remained. The acute pain had almost ceased. I then strapped carefully each finger with straps of linen moistened with an isinglass cement, which I use for this purpose, and confined the hand and wrist in an immovable case of the same material, leaving the upper surface of the wrist open for the dressing of the sore and the use of the caustic when necessary. In this instance the use of the potassa cum calce was repeated but once, and that slightly. The ulcer healed up firmly, all pain ceased, and in three months the bone disease was evidently cured, the patient only having to contend with the stiffness of the fingers and the pain inseparable from the attempt to restore their motion. From this his recovery was slow, so that a year elapsed before he was able to completely close his hand, but he ultimately recovered its perfect use.

In the month of September, 1864, H. H., aged fifteen, after over-fatigue, was attacked with pain in his right foot, and became unable to walk. He was treated, for two months, in the country,

and was told that his case was only a bad sprain. I saw him on the 1st of November. He was then quite unable to bear on the foot, which was pale, œdematous, and had altogether lost its healthy aspect. The disease was confined to the metatarsal bone of the great toe, and there was evidence of the formation of matter under the fascia. Having decided on using the caustic, I made a large eschar over the suspected spot on the dorsum of the foot, and incising its centre, I at once carried the caustic deeply down to the diseased bone; an escape of thin matter took place, relief from pain followed almost immediately. The separation of the slough occurred in fifteen days, leaving a large free circular opening down to the diseased bone. November 20th.—There was a puffiness observed on the inner surface of the foot, I therefore made a second free opening with the caustic down to the diseased bone, and afterwards at intervals of ten days or a fortnight, I used the caustic slightly to obviate the great tendency in the orifices to heal; at the same time the openings were dressed deeply with stimulating ointments. The discharge gradually ceased, the ulcers healed from the bottom in the most satisfactory way; and early in May the patient returned to the country, able to bear on the foot without pain, and having every prospect of a perfect recovery.

Anne Dowd, aged 25, admitted into Hospital January 12th, 1865. Her right leg was greatly swollen from the knee down; over the inner ankle there was a large gangrenous swelling, very like an anthrax; and along the course of the tibia were several fluctuating tumours in a state of high inflammation; over the external malleolus there was also an abscess, and the apprehension was entertained that the disease communicated with the ankle joint. The least motion of the foot produced agony. The imperfect history obtained from the patient was to the effect that twelve years back she had been confined to bed for two years with necrosis of the affected limb, but that she had made a good recovery, and remained well until a fortnight before admission, when in carrying a heavy load she received a wrench that was followed by inflammation and violent pain above the inner ankle, where an abscess followed, and the inflammation extended up the leg. This case was so unpromising at first, that the question of amputation was canvassed, and for several days I treated it as I would a compound fracture, keeping the limb well supported, and raised above the level of the body. When the great swelling had in some measure subsided, there remained considerable enlargement of the tibia, and

along its surface were four collections of matter, in addition to the large sloughy opening that seemed to communicate with the ankle joint, and into which a finger could pass for fully the depth of an inch.

On the 5th of February I put this patient under chloroform, and used the caustic freely over the thinned and diseased skin, making five large eschars, which on their separation left free openings down to the bone.

February 23rd.—Marked subsidence of pain, inflammation, and swelling. The two upper openings nearly closed from the bottom. The three lower openings not healing up in such a satisfactory manner, I therefore put the patient under chloroform, and used the caustic again, destroying all diseased and undermined integuments, and rotating the solid stick of potassa cum calce as deeply as I could into the diseased openings.

March 20th.—A small piece of bone came away from lower opening.

25th.—Patient can move the ankle joint slightly in the direction of the limb, but no lateral motion is permitted. All the openings healing up from the bottom in a healthy manner. Patient getting about the ward on crutches.

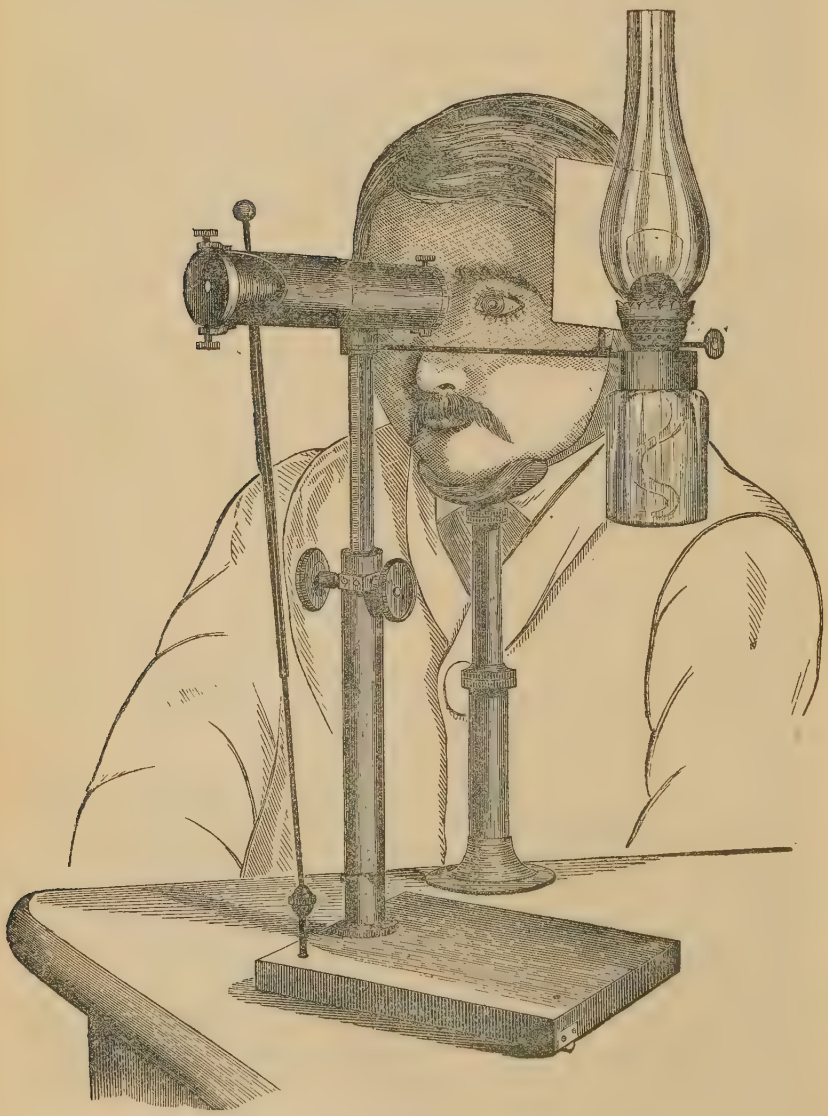
Left the Hospital on the 14th of April, walking with the help of a crutch. All the openings firmly closed up.

Art. IX. — *On Smith, Beck, and Beck's Modification of Dr. Liebreich's Ophthalmoscope.* By E. Perceval Wright, M.D.; Ophthalmic Surgeon, Dr. Steevens' Hospital, Dublin.

During the Winter of 1864, when engaged in lecturing on some of the diseases of the eye, I found it impossible to demonstrate to several of my pupils certain morbid changes in the retina, on account of their want of practical acquaintance with the use of the ordinary hand ophthalmoscope. In this emergency I thought it advisable to employ an ophthalmoscope which would enable me to show, with some degree of certainty, not only the ordinary appearances of the retina, but also the same appearances to several students in succession. For this purpose I wrote to my friend Mr. R. Beck, of the firm of Messrs. Smith, Beck, and Beck, of London,

suggesting to him to make for me a Liebreich's ophthalmoscope, or to modify it so as to make it more portable. This he most kindly undertook to do, and he forwarded to me, in a very short time, the instrument which I proceed briefly to describe; one which I think, when known, will be very extensively used in this country.

Liebreich's ophthalmoscope consists of two short tubes, moving



one over the other by rack and pinion. The tube nearest to the observer has a piece cut out of its side at the free end, where hangs a small metallic concave mirror on trunnions, in such a manner that it turns readily on its vertical axes, and can be removed with facility. The other tube, turned towards the patient, carries at its free end a convex lens of about two inches focal length, hung in the same way as the mirror. This latter tube is securely united to the upright stem. The optical arrangement is the same in the instrument figured as in Liebreich's, but the mechanical construction is simpler; it can be used on a table without fixing; the adjustments are easily and quickly made; and owing to the lamp being attached to the stem of the ophthalmoscope by an arm, the illumination remains unaltered however much the instrument may be moved about during the necessary adjustments.

The chin rest is detached and will stand of itself, the height being regulated by a sliding tube, which may be fixed in any position by the milled ring. The ophthalmoscope is mounted upon a mahogany board, which has three small rollers underneath, so that the whole of the instrument may be moved in a direct line, nearer or further from the eye, at the same time allowing of any side movement that may be required, or of a slight rotation of the instrument, which is sometimes necessary when light is reflected from the surface of the cornea. The height of the horizontal tube carrying the mirror and the lens, is regulated by the milled heads which are connected with a rack and pinion. The mirror, when in use, is drawn out about an inch from the upright stem; and the lens is so rotated as to throw aside the reflexions from the surfaces of the glass. The lamp is attached to the stem by means of an arm; a shade fits into a hole near the lamp, and prevents the direct light from shining on the patient's eye. The object upon which the eye to be examined should be fixed, consists of a small knob of ivory, which can be moved in any direction by means of its sliding tube and the ball and socket joint at its base. This ophthalmoscope packs into a very small case, and can be carried about with the greatest ease. The best oil to burn in the lamp is some of the best refined paraffin oil.

In all cases when it may be deemed expedient to use a fixed ophthalmoscope, I have no hesitation in asserting that this improved instrument of Messrs. Smith, Beck, and Beck will be found one of the very best. The workmanship and finish leave nothing to be desired.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Personal Responsibility of the Insane. By JAMES F. DUNCAN, M.D., T.C.D., &c. Dublin: Fannin and Co. 1865.

DR. DUNCAN has been long and favourably known to the profession in connexion with the medical treatment of insanity; and his special experience has been such as to entitle any literary production of his on this subject to more than ordinary attention. The work before us, published in Messrs. Fannin's usually good style, comprises 98 pp., small 8vo; and we feel bound to say that a perusal of them will greatly interest the scientific reader, and this quite as much as the general public, for whose information this essay is more particularly intended.

In the opening part of the essay Dr. Duncan discusses the difference between insanity and crime, and observes:—"What, then, is the difference between insanity and crime? It is simply this—insanity is a morbid condition of some part of the material organization connected with the mind, while crime is a perverted action of the mind itself. The one is properly a disease, and belongs to the province of the physician; the other is a change independent of disease, and belongs to the province of the moralist." The essence of insanity in every form, shade, and variety, is defined to be "some deviation or other from the healthy condition of the corporeal organization which disturbs the free action of the mind in some of its faculties, and renders it incapable of discharging its functions in a proper manner."

We shall give his own description of what is, perhaps, the most prominent idea in Dr. Duncan's essay. In reply to the question—Where is the line to be drawn which divides the region of responsibility from that of irresponsibility? he observes:—"Two very opposite opinions prevail upon this point—one is that every form and degree of insanity necessarily excludes the idea of moral accountability in the offender; the other that nothing but actual frenzy, or the total loss of intelligent consciousness, produces this

immunity. The first opinion is commonly entertained by the masses of society, including generally the members of the medical profession; the second is more commonly held by legal writers and by judges on the bench."

He proceeds to show that in practice the former opinion is generally acted on in our courts of justice; while on the other hand, the decisions on cases similarly circumstanced are various and conflicting. Further, we find an elaborate discussion on the divisibility of the mind into faculties; and on this matter Dr. Duncan makes no *ex parte* statement, but carefully weighs the plausible arguments usually advanced on both sides of the question. Respecting this part of the subject he observes:—"The settlement of such questions as these is of the greatest importance in reference to the subject discussed in these pages. If the opinion be correct, that the mind is indivisible, and that it must act as a whole whenever it acts at all, it will necessarily follow, as Lord Brougham has asserted, that there can be no such thing as partial insanity; the mind, if unsound in any respect, must be considered unsound in all its operations, even when acting to all appearance in conformity with the requirements of sound mind." Again, he observes:—"The generally received opinion on this subject, in the present day, notwithstanding the dissent expressed by the learned authorities to whom I have alluded, is that the mind, like the body, consists of a variety of distinct faculties, corresponding in number to the different functions they have to perform. A careful study of its operations in health and disease, as well as analogy drawn from the structure of the body, leads us to this conclusion; yet for the reasons stated it can be looked upon only as a matter of inference and not as a truth established upon evidence, not to be set aside." Dr. Duncan proceeds to explain how it is, that in cases of alleged partial insanity medical men so frequently give conflicting testimony. This he shows to proceed from the simple cause that one man who examines the case is put in possession of the clue to the delusion under which the patient labours; *he* immediately arrives at one conclusion. A second medical man, not having been put in possession of the precise point upon which the partial derangement turns, after a long and close conversation with the patient, comes away with the impression that the suspicion of insanity is unfounded.

We have neither time or space to follow Dr. Duncan through that large part of his volume which treats of the legal test of

insanity—"Knowing right from wrong." Suffice it to say that he shows the said test to be difficult of application in most cases, while it is so vague as to be practically useless in determining the point for which it is relied on.

He also ably refutes the dictum of Lord Westbury, that insanity is a fact which can be ascertained by evidence like any other fact.

We now come to review the latter portion of the essay, which is divided into sections, the first of which is headed —"Of insanity Characterized by Delusions." In reply to the question—What is a delusion? Dr. Duncan observes:—"There are three distinct features that every delusion ought to possess in order to its being considered evidence of an unsound mind. It ought, in the first place, to exhibit a certain degree of permanence. . . . . In the second place, it must be proof against conviction, so that arguments which are sufficient to satisfy ordinary minds of its erroneousness, fail to change the views of the person who holds it. And lastly, it must influence the conduct of the individual. Without this latter characteristic, however absurd in itself, its harmlessness will prevent its being regarded as an evidence of mental derangement."

Under the next sectional heading—"Of Moral Insanity," Dr. Duncan very truly remarks:—"The name given to this form of the complaint is singularly infelicitous." Doubtless it is, because, as afterwards remarked, it suggests the idea of vice and depravity as an essential feature of the species; whereas many persons affected by it are blameless in their conduct, and exhibit sound judgment upon all questions of morality. What is to be understood by the term is "a perversion of the moral feelings, including the affections, passions and instincts, as apart from and unconnected with any lesion of the intellectual faculties."

Distinct from that habitual want of cheerfulness which distinguishes many of the subjects of chronic lunacy, Dr. Duncan gives a section of his essay to "Insanity Characterized by Depression of Spirits." This he considers to be frequently an acute affection, temporary in its duration, and recurring at intervals in the same individual. There is also a lengthened section devoted to the discussion of "Impulsive Insanity," as well as one on "Insanity Characterized by Various Morbid Propensities." These, Dr. Duncan properly observes, should be included under the general head of moral insanity, to which such complaints as pyromania and dipsomania are generally considered to belong. We shall not here pause to discuss the important social bearings of dipsomania

in connexion with moral insanity; as in a former number of this Journal the entire subject has been treated in a review entitled "Reformatories for Drunkards;"[a] we may, however, state that Dr. Duncan treats them with that ability and moderation which we look for in vain among the riders of hobbies, especially among such as are monomaniacs on the subject of total abstinence from spirituous drinks. The last section, headed—"Of Coroner's Inquests," is a most sensible protest against the indiscriminate verdict of "temporary insanity," which mistaken charity almost invariably pronounces in cases of suicide. Dr. Duncan clearly shows that the knowledge that such verdict will almost certainly follow a suicide, is most likely to induce an otherwise hesitating man to commit this fearful crime. We shall give whole and entire Dr. Duncan's *conclusions* regarding the subject of his essay:—

"I. Persons labouring under ordinary mania, where there is incoherence, excitement, and the general symptoms of complete mental estrangement, are thereby rendered entirely irresponsible for whatever criminal actions they may commit in that state.

"II. Persons labouring under the various forms of partial insanity are irresponsible for the criminal actions they may commit, which are plainly traceable to that condition, even though such actions may not admit of justification, on the hypothesis of the causes which provoked them being real instead of imaginary.

"III. When criminal acts are committed by persons partially insane, although no connexion may be traceable between these actions and their insanity, their condition may be looked upon as a ground of mitigation of punishment, and treated accordingly.

"IV. When persons convicted of crimes are exculpated, either wholly or partially, on the ground of insanity, their condition requires them to be transmitted to a proper hospital or asylum for their immediate medical treatment.

"V. That, subject to proper precautions, as soon as such patients are restored to health, reason and humanity require that they should be set at liberty, unless there should appear to be special risk of a relapse.

"VI. That when unoffending persons have suffered personal injury from the violence of insane persons, reasonable compensation should be made to them out of the lunatic's estate.

"VII. That Coroner's Juries should not be expected, or required to

[a] See the Number for May, 1862. As its author has since then had it reprinted as a pamphlet, with his name affixed, we may add, that it was written by Dr. Belcher, a frequent contributor to this Journal.

append to their finding in cases of suicide, the usual formula, 'when labouring under temporary insanity,' unless some urgent necessity arise to require the person's state of mind to be carefully investigated, and the fact of such condition existing has been clearly established.

"VIII. That unless in clear cases of positive mental derangement some modified form of the obsolete mode of un-Christian sepulchre should be maintained as a means of checking the too prevalent tendency to suicide."

Shall we criticise the style of this essay? In our opinion it is remarkably well written, and in no instance does the writer fail to convey his meaning in concise, plain, and intelligible words.

We have noted but two slight verbal inaccuracies. On p. 61, writing of *three* characteristics of delusion, he speaks of the *third* as "this *latter* characteristic," meaning, of course, the *last* of three. Again, in the section on coroner's inquests and in conclusion VIII., as above given, the word "sepulchre" is curiously used; and is doubtless intended for "sepulture."

These trifling words, however, tend to show how well written the essay is, as the exception proves the rule. We commend it heartily to the profession and to the educated public. Its style, composition, and mode of arrangement is such as we should expect from a scarlet gownsman of our National University; and its matter is in keeping with the motto of our Irish College of Physicians, of which Dr. Duncan is a Fellow, namely—"Ratione et Experientiâ."

The Works of Sir Benjamin Brodie, Bart., D.C.L., Serjeant Surgeon to the Queen, President of the Royal Society, &c., with an Autobiography. Collected and arranged by Chas. Hawkins, F.R.C.S. Eng. In 3 Vols. London: Longmans. 1865. 8vo, pp. 659, 680, 697.

In the Introduction to this edition of the works of the late Sir Benjamin Brodie the editor tells us that he considers the publication of a collected edition of his works the fittest memorial of his friend and teacher. "No public memorial has yet," he says, "been raised to this great surgeon. When the Wellington Despatches appeared a distinguished foreigner remarked, 'What a noble monument Colonel Gurwood has raised to the Duke—one more enduring than

marble or brass.' The task of arranging such a monument to the best of friends and most indulgent of instructors has been to me a labour of love." We cordially agree with Mr. Hawkins that such a monument is the most worthy of the fame of Sir Benjamin Brodie, and the best calculated to stimulate others to adopt the course that led him to the highest pinnacle of social and professional reputation, gained for him the admiration and esteem of all his contemporaries, and made his name one that must be known so long as human flesh has ills to bear, and wherever the healing art is cultivated or the sciences on which it rests are studied.

The works collected by Mr. Hawkins embrace Sir Benjamin's earliest as well as his latest writings, and all have their value; for, though some of the earlier physiological essays may fall short of the present state of science, they were very important advances on that of the period when first published, and the facts established served as firm starting-points whence further researches took their start. The works of Sir Benjamin Brodie are, however, too well known to require any notice here; we shall rather devote the space at our command to a brief sketch of the very interesting autobiography with which they are introduced.

Many of the successful men whose lives have been written have been men who have risen to eminence in spite of the most adverse circumstances; but the history of Sir Benjamin Brodie is that of a man surrounded by the most favourable circumstances; his great merit consisted in his having cultivated all the advantages he was possessed of. Instead of letting these lessen his own individual efforts, he regarded them as stimulants to exertion, and used them as stepping-stones to further progress. Sprung from a father whose great talents and acquirements seem only to have been surpassed by his judgment, he received his early education at home; and, while instruction was not neglected, education in the true sense of the word was the great object, for, in addition to a good knowledge of classics and the elements of mathematics, chemistry, and other sciences, he was led to form habits of application and industry, of reflection and self-reliance.

Prepared by such a training, and firmly impressed with the knowledge that his livelihood depended on his own exertions, young Brodie proceeded to London in 1801, being then in his eighteenth year, to enter on the study of surgery, in obedience to his father's wishes, who was led to guide his mind to that profession by the great success of Dr. Denman, who was married to his sister, and of

Dr. Baillie and Sir Richard Croft, who married his nieces. In alluding to this important era in his life Sir Benjamin takes occasion to express his opinion as to the special callings to certain ways of life that some young men are supposed to have. Like ourselves he has no faith in these; he regards them as mere fancies which are liable to give way to other fancies with as little reason in them as they themselves had when they first began to exist. The persons who succeed best in professions are, he says, those who persevere as a matter of duty; they often feel their new pursuit to be unattractive enough in the beginning, but as they go on and acquire knowledge the case is altered, and then they become interested in what they are about. We believe this matter of likings and dislikings to be the cause of the wrecking of the fortunes of many young men, who if they entered on their work with earnestness would soon find pleasure where all seemed irksome. In Sir Benjamin's case his first teachings had a great effect in fixing his taste.

"During my first season in London, I attended Mr. Abernethy's lectures on Anatomy. He was an admirable teacher. He kept up our attention so that it never flagged, and that what he told us could not be forgotten. He did not tell us so much as some other lecturers; but what he did, he told us well. His lectures were full of original thought, of luminous and almost poetical illustration, the tedious details of descriptive anatomy being occasionally relieved by appropriate and amusing anecdotes, which, though they had been repeated over and over again, as one course succeeded another, were very agreeable to us new-comers. Like most of his pupils, I was led to look up to him as a being of a superior order; and I could conceive nothing better than to follow in his footsteps; and thus I was led to regard the department of the profession to which he belonged as that to which I should belong myself. Of this conclusion I have never since had reason to repent; and after an experience of fifty years, I am confirmed in the opinion that the pursuit of what is called pure surgery, such as it is in large cities, in connection with a hospital and a medical school, is more replete with interest, and, on the whole, more satisfactory, than any of the other branches into which the *ars medendi* is divided."

Among his earliest associates was Lawrence, who still survives him, and of whom he gives the following sketch:—

"From that time to the present, Lawrence and myself have been moving in parallel lines, he having had the largest share of private practice next to myself; and it may be regarded as somewhat to the credit of

both of us that there has never been any manifestation of jealousy between us. I have already mentioned that, when a young man, he had some faculties in great perfection, and he has them still, but little (as far as I can see) impaired by the addition of fifty years to his age. He has a great memory, and can readily recur to, and make use of, what he knows. He has considerable powers of conversation, but without obtruding himself to the exclusion of others, as is the case with too many of those who are reputed to be good talkers. What he says is full of happy illustrations, with, at times, a good deal of not ill-natured sarcasm. In public speaking, he is collected, has great command of language, and uses it correctly, but not equal to what he is in the ordinary intercourse of society. In writing his style is pure, free from all affectation, yet in general not sufficiently concise. His reading has been extensive: he is well acquainted with modern, and moderately so with the ancient, languages. His professional writings contain a vast deal of information, but it is more as to what he has taken from other authors than as to the results of his own experience and observation. That he is thoroughly acquainted with his profession cannot be doubted, for it would not have been possible for him otherwise to retain for so long a period the high place which he has occupied."

His eldest brother, who was preparing for the bar, was also studying in London at this time; he and Sir Benjamin lodged together, and they were thrown into the society of men older than themselves, and of refined and cultivated tastes, many of them already beginning to be known, and destined to become distinguished in their several ways. Through the influence of one of these, Dr. Maton, he was elected a member of the "Academical Society;" and even at this early period in his career he read papers on the benefits of metaphysical enquiries and on the mode of conducting scientific researches, showing that, though he was working hard at anatomy and chemistry, he found time for other studies; and it is interesting to observe that his earliest paper and the last essay he ever published were on the same subject—Psychological Enquiries.

During his second winter, by the advice of Dr. Baillie, he spent some time in a chemist's shop, to gain some knowledge of materia medica and the making-up of prescriptions; and in the spring of 1803 he first entered as a pupil, under Mr., afterwards Sir Everard, Home, at St George's Hospital.

"The commencement of my studies at the hospital was that of a completely new era in my life. Hitherto it is true that I had worked hard enough. With the exception of Lawrence, I doubt whether any one

of my acquaintance had been equally diligent. But it was rather as a matter of duty, or rather I ought to say of necessity, than because I felt any very great interest in what I was doing; and most willingly, if I could have afforded it, would I have turned my back on anatomy and returned to literary pursuits. A great change took place as soon as I became familiar with the business of the hospital.

"To those who really desire to learn, the wards of a hospital are soon found to be replete with interest. At first all is confusion. The nice distinction of symptoms on which the diagnosis of disease depends, why the pulse in one case indicates immediate danger, and in another none at all, why one patient recovers and another dies, why the same kind of treatment is successful in one instance and fails in another; these, and a multitude of other matters, are quite inexplicable to the young student. Everything is seen as it were through a mist. After no long time, however, the mist begins to clear away, and whoever has advanced thus far finds no difficulty afterwards. Every case is an interesting subject of enquiry. A great game is being played in which the stake is often neither more nor less than the life or death of a fellow-creature, and in which those among the students who devote themselves to their business perform a humble yet not unimportant part without any painful feeling of responsibility. Not many months elapsed before I became sensible of the good effect of these new studies, and of the wisdom of Dr. Baillie's advice that I should make myself a tolerably complete anatomist before I commenced my attendance at the hospital; as I found that I was able to comprehend many things that were passing under my observation which I could never have properly comprehended otherwise, and in which those who were less prepared in this respect were little able to understand.

"During the summer of 1803 I never failed to pass the early part of the day in the wards of the hospital. In the afternoon I usually dined by myself at my lodgings in Knightsbridge, and in the evening read some Latin classics, and other books which formed my scanty library, or a novel from a small circulating library at Brompton, or walked in Kensington Gardens. As the season advanced, most of my friends left London. A few, however, remained, whom I met occasionally; among them was Dibdin, since known by his works on Bibliography, who at that time resided at Kensington, not very far from my lodgings at Knightsbridge, and with whom I occasionally wandered to hear the nightingales in the lane beyond Holland House."

. . . . . .

"During the summer of 1804, a friend of mine, of the name of Jeffreys, was house-surgeon of the hospital, and my intimacy with him enabled me to pursue my studies there with great advantage. He had more knowledge of his profession than most young men of his standing. In

the early part of the day, I was always with him in the wards; and in the evening, we were generally together. It was from him that I first learned the importance of keeping written notes of cases, a practice which I continued ever afterwards. These notes I have carefully preserved. They now form many thick quarto volumes of manuscripts, to which (and even to the earliest of them) I not unfrequently refer with advantage, even at this advanced period of my professional life. My custom has been to take short notes at the bedside of the patients in the day, and to expand them, with the aid of my memory in the evening. Thus they became an exercise of the memory, and, instead of weakening, tended to strengthen that important faculty. After an experience of nearly fifty years, I am satisfied that no one can be well acquainted with his profession, either as a physician or surgeon, who has not studied it in that manner. It is only by these means that a case can be thoroughly and scientifically investigated, or that that minute and accurate knowledge of it can be obtained which is necessary to a right diagnosis. For one who is to occupy hereafter the situation of a consulting practitioner, to whom younger or less experienced persons will apply for assistance in cases of doubt or difficulty, it answers another purpose, as it enables him to express himself with greater facility, and especially to give written opinions with a degree of clearness and precision with which he could not give them otherwise. I have always, during the many years in which I was a teacher, and a hospital-surgeon, endeavoured to impress on the minds of my pupils the necessity of making and preserving such written records of their experience; and I have often been pained to observe how small a proportion have followed the advice which I gave them. Some of them find a difficulty in doing so from the want of original education, and really not having a sufficient knowledge of the use of language even for this simple kind of literary composition; others neglect it from mere idleness; while the great mass of students, whose period of professional education is limited, are so occupied by the great (and, as I think, unnecessary) number of lectures which they are now required to attend, and in running from one class-room to another, that they really have neither the leisure nor the physical powers necessary for pursuing, in any efficient manner, the practical study of disease in the wards of the hospital."

Looking back at this period of his life, Sir Benjamin makes remarks on the value of lectures and methods of education too much in accordance with the views we have, on several occasions, given expression to in these pages to be passed over in silence:—

"I must not pass over this part of my life without noticing a very great advantage which I possessed during the period of my professional

education, compared with what I should have had if I had lived in these later times. No rules were then laid down as to the number of lectures which I was required to attend. The examination at the College of Surgeons was sufficiently good, as far as it went, but it was of a very simple and elementary kind. It was no more than a diligent student might pass without any special preparation for the purpose. The consequence was, that I was enabled to take my education very much upon myself; and I soon found that I could no how obtain so much useful knowledge as by a diligent attendance on the dissecting-room, and on the wards of the hospital. I cannot say that I neglected the use of books, but it was more in the way of reference and illustration than by a regular course of reading. I attended lectures on Anatomy, and, during one season, Dr. Crichton's lectures on the Practice of Physic, Materia Medica, and Chemistry, the latter especially with some advantage. During my first season in London, I had entered as a pupil to Mr. Abernethy's lectures on Surgery; but having at that time seen no surgical practice, I did not understand them, and soon ceased to attend them. I afterwards entered to some other lectures on Surgery, at the West-end of the town, but found that I learned nothing from them, so I ceased to attend there also. Mr. Home was accustomed to give an annual course of twelve surgical lectures gratuitously to the pupils of the hospital. These were excellent, and I attended them, year after year, with great advantage. Altogether, I do not suppose that I attended one-fourth of the number of lectures which the unfortunate students are now required to listen to under the direction of the constituted authorities. But I was acquiring knowledge in other ways, and much more substantial knowledge than can be acquired from such dull and hum-drum discourses as lectures usually are; and, which is better still, I had leisure to make my own observations, to think and reflect. Nor was this style of education peculiar to myself. I remember when Mr. Abernethy complained that Lawrence would not attend lectures. My friends and contemporaries, Jeffreys and Lawrence, took the same course; and so it had been with Nicolson, who was some few years in advance of us. I can easily conceive that, if I had been compelled to sit on the benches of a theatre four or five hours daily, or tempted to compete for prizes as students are, and to get crammed for various examinations, my position in life afterwards would have been very different from what it has been in reality."

He had now been house surgeon to St. George's Hospital, an office filled by one of the better informed pupils, and which may be held twelve months, but Sir Benjamin was induced to resign it after six months, by an offer of a demonstratorship of anatomy under Mr. Wilson; and he was employed at the same time by Home to assist him in his private operations, and to help to carry

out his inquiries in comparative anatomy; and about this time he became acquainted with Sir Joseph Banks, who proved a most valuable acquaintance.

Fortune hitherto so lavish in her favours, finding all her gifts so well taken advantage of, now determined to mark Sir Benjamin as her own, by bestowing one more important still, and accordingly in March, 1808, before he had completed his twenty-fifth year, he was elected assistant surgeon to St. George's Hospital; and as one of the surgeons was absent for some nine years serving with Wellington on the Peninsula, he had entire charge of his patients. His hospital duties now occupied a great part of his time. The custom at this period was for the surgeons to go round the wards on two days of the week only, not attending otherwise except when there were operations or severe accidents to be attended to; but Sir Benjamin and his colleague, Mr. Robert Keate, adopted the plan of attending daily and superintending everything in urgent cases, visiting several times a-day. Sir Benjamin moreover began to give clinical lectures, the first of the kind ever delivered in a London hospital. He also began now to lecture on surgery at the school; and very popular and well attended his lectures were.

Hitherto he had been living in lodgings, but he now took a house, 22, Sackville-street, placed his name on the door, and began to think seriously of private practice, obtaining in the following year from this source between £200 and £300. This with his other resources made him free from anxiety, and he was able in 1810 to engage with considerable interest in some physiological inquiries on his own account, the results of which formed the subjects of two papers communicated to the Royal Society, of which he had been elected a Fellow; one "On the Influence of the Brain on the Action of the Heart and the Generation of Animal Heat;" the other "On the Effects Produced by certain Vegetable Poisons." The first of these had formed the subject of the Croonian Lecture in 1810, and obtained for him the Copley gold medal. At this time he was only twenty-eight years of age; and he records, with much pride, that when the question of giving him the medal was discussed in the council, the only objection raised was that it had never been given to so young a man; to which Dr. Wollaston said, that he thought if he deserved the medal that was only an additional reason for giving it to him

During this time his private practice was gradually increasing at the rate of £200 to £250 a-year; and he was continuing his

physiological investigations, and entering on his study of the diseases of joints, on which he communicated a paper to the Medico-Chirurgical Society in 1813. In 1812 changes in the school led to his giving up lecturing on anatomy; and, writing after the lapse of forty years, he refers to the familiarity with anatomy, derived from his teaching it during seven years, as having been of the greatest service to him.

In 1814 his health began to suffer from unceasing occupation of mind and body for so long a period, and partly from having been in London for ten years, and never breathing the air of the country for more than two or three days at a time, and then only at rare intervals; and his friends began to speculate as to his soon making a vacancy at St. George's Hospital; but a visit to the sea side soon restored him to health; and in 1816 he married the third daughter of Serjeant Sellon, then but nineteen years of age, he being thirty-three. At this time his professional income derived from fees and lectures amounted to £1,530, concerning which he makes the following rather *naïve* remarks:—

"I had previously saved sufficient money to refurnish and paint my house, and in other ways make it more fit than it had been before for the reception of a bride. I now, for the first time, had a carriage and pair of horses. In other respects, we made very little addition to my former establishment. As my wife had no fortune given her at the time of our marriage, nor indeed any except what had been settled on her after her father's and mother's deaths, and as my profession entailed some expenses on us, we were under the necessity of being careful as to our mode of living. My dear wife had no expensive habits, and we managed to make both ends meet at the end of the year. Still, I cannot but say that this was a period of considerable anxiety, when I felt for the first time that another individual as well as myself, and probably children hereafter, had to depend, not only on my professional character, but also on my bodily health. Fortunately, in the beginning of the following year there was a more manifest increase of my practice than there had ever been before. This kept my anxiety within bounds; still it was considerable, and was probably the cause of my having some return of the dyspeptic symptoms under which I had laboured formerly, and which continued to trouble me, from time to time, for the two or three following years."

In a year or two, however, notwithstanding the recurrence of the dyspepsia from his former rashness, we find him incurring fresh expenses, but without any injurious effect on his health:—

"During the first two or three years after our marriage we continued to reside in the small house in Sackville Street in which I had resided previously. In the beginning of the year 1819, however, I took a house of greater pretensions in Savile Row, and we remained in it until we removed to a larger one in the same street, which we still inhabit. As my income had been steadily increasing, I felt myself to be guilty of no imprudence in making this change, and the event justified me in doing so, as my income in 1819 exceeded that of the previous year by more than £1,000. This increase may be in part attributed to the publication of the first edition of my work on 'Diseases of the Joints,' which had taken place in the previous year. Other circumstances, however, contributed to it. Although I was no more than thirty-six years of age, my name had been for several years before the public. Sir Astley Cooper, who had succeeded to the large practice of Mr. Cline and the smaller one of Sir Everard Home, too confident of his position, had already begun to lose some of the vast reputation which he had previously enjoyed. Some one else was wanted, and I was ready to fill the vacant place. From this time my practice steadily increased, so that almost every year made considerable additions to it. Hitherto my income had been little more than sufficient to meet my annual expenditure, but I now began to lay by a considerable portion of it; and finding that I had the prospect of providing for my family, and of acquiring in the course of no very long time a moderate independence, I was relieved of much of the anxiety which I had formerly experienced."

Though Sir Benjamin appears to have thought Sir Astley Cooper's reputation declining at this time, it appears from the next extract we shall make that Lord Liverpool did not take the same view:—

"It was in the year 1821, and while I held the office of professor at the College of Surgeons, that I was first called on to attend the king, George IV., under the following circumstances. His majesty had one of the common encysted tumours which occur on the scalp, which was large enough to be troublesome to him. He showed it to Sir Everard Home, who advised him to have it removed by an operation. The king was anxious to undergo the operation. His majesty, however, expressed to Sir William Knighton that he wished the operation to be performed by myself, Sir Everard being, however, present, and Knighton was commissioned to make this communication to me. I cannot say that I derived any particular satisfaction from it, as I found that I had already obtained the patronage of the public, and was quite contented with it. In the meanwhile, however, the subject of the proposed operation was mentioned to Lord Liverpool, who was then prime minister. Lord

Liverpool represented to the king that it was a matter which might concern the public as well as himself, and urged that nothing should be done without Sir Astley (then Mr.) Cooper being first consulted, and that, if an operation was determined on, that Sir Astley should perform it. Sir Astley being at that time the most conspicuous person in his profession, I cannot doubt that Lord Liverpool's judgment was quite correct. Accordingly, Sir Everard Home, Sir Astley Cooper, and myself were summoned to Windsor; when, after examining the tumour, we agreed that nothing but an operation could be of any service, and that it should be performed when the king returned to London. Mr. Cline was consulted afterwards, who confirmed the opinion which we had given. Eventually the operation was performed by Sir Astley Cooper, in the presence of Sir Everard Home, Mr. Cline, Sir William Knighton, the king's physicians, Sir Henry Halford, Sir Matthew Tierney, and myself, making indeed a very large assembly for so small a matter. After this attendance, Cooper was created a baronet, and Sir Everard Home was comforted by being appointed to the office of surgeon to Chelsea Hospital, vacated by the death of Mr. Thomas Keate, and by his son, who was then a very young lieutenant in the navy, being advanced rather prematurely to the rank of commander. From this time, when any surgical operation was required, the king, for some years, was in the habit of applying to Cooper; but on some special occasions I was summoned to meet him in consultation, though I held no actual appointment in the royal household until the year 1828, when, on Sir Astley having been appointed Serjeant-Surgeon, I was gazetted as surgeon to his majesty's person, in his place."

In 1834 Brodie was created a baronet, which served as a further incentive to exertion that he might be enabled to leave his successor an income more worthy of the title; but we must now hasten to conclude our sketch. We regret that we cannot give more extended notices of Sir Benjamin's criticisms on his contemporaries, which are full of interest and instruction, as showing the materials out of which successful practitioners are made; but for these we must refer to the autobiography itself, which has, we find, been published in a separate form. It closes with his resignation of the surgeoncy of St. George's Hospital in January, 1840. He held the office of assistant surgeon for fourteen years, and of surgeon for nearly eighteen. In resigning he was influenced by various motives; he wished to lessen the amount of his labours, and was unwilling to have it said of him, as he had heard it said of others, that he retained an office of such importance and responsibility, when either from age or indifference he had ceased to be fully equal to the

duties belonging to it; and lastly, with a feeling that we would commend to the notice of others, when he saw intelligent, and diligent, and otherwise deserving young men around, waiting their turn to succeed to the hospital appointments, it seemed to him that there was something selfish in standing longer in their way, when as far as his own merely wordly interests were concerned he had obtained all that he could desire.

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1. *The Second Step in Chemistry; or the Student's Guide to the Higher Branches of the Science.* By ROBERT GALLOWAY, F.C.S. With Illustrations on Wood. London: Churchill and Sons. 8vo. 1864. pp. 771.
2. *A Manual of Chemical Analysis, Qualitative and Quantitative, for the Use of Students.* By HENRY M. NOAD, F.R.S. London: Lovell Reeve, and Co. 8vo. 1864. pp. 663.

ALTHOUGH thus grouped together, and to a hasty glance upon similar subjects, these two works, both excellent of their kind, are of widely different scope, and aim at discharging offices of very dissimilar character indeed in the students chemical education. In the following notice we shall endeavour to point out to our readers what kind and amount of assistance they may expect to derive from either or both of them.

The student who has already mastered the elementary details of chemistry, and who is anxious still further to extend his knowledge, will find a rich treat awaiting him in Mr. Galloway's work; but it is as well for him to know, that without a tolerably accurate knowledge of the elementary facts in chemistry to attempt the perusal of this work would be indeed utter loss of time; possessed of this knowledge, however, we know no work in which he will find a fuller, and, at the same time, a plainer account of the more advanced departments of chemical science than in the work before us. In it will be found, fully recorded, the labours of Sir B. Brodie, the illustrious son of an illustrious father; Odling, Gibbs, Gerhardt, Laurent, Williamson, Hofmann, Bunsen, Kirchoff, Wurtz, Playfair, Kolbe, Frankland, &c.—labours of rare value in themselves, tending as they do to revolutionize, in a few years, our present ideas upon various subjects in chemistry—but frequently inaccessible to the majority of students in their original form,

inasmuch as they are scattered over the world of literature in monographs, periodicals, &c.; many of them, also, being written in languages with which, probably, they are not familiar. Were this the extent of Mr. Galloway's achievements, it would be sufficient to secure for his work a high meed of praise as a laborious collection of the theories and facts of these distinguished investigators in the higher walks of chemical science; but, in addition, he gives us the results of his own experience, and also introduces into each chapter, after the enunciation of each principle, *exercises* by which the student is enabled to test whether he has mastered the details which he has just been reading, and by which excellent plan his mental powers are stimulated to healthy activity. If any system can ever eradicate that destructive and withering system of medical education of modern growth—*cramming*—it is this. By it only can the student ever arrive at his intellectual legs, and be able to walk alone, not dependent on the pernicious plan of mental spoon-feeding that is the bane of our present students, and which, in years to come, they will so bitterly regret. Although apparently foreign to the subject we have in hands, yet these considerations are forced upon our consideration by the excellence of the course pursued by Dr. Galloway in the work before us, and which, perhaps, will be best understood by our readers having submitted for their perusal an example. Such may be taken at hap-hazard from any portion of his work, for almost in every page of it is this excellent plan fully carried out.

"1. Specific Gravity of Gases and Vapours.—Gases and Vapours differ in their densities, or specific weights.

"2. Atmospheric air, at 60° F., and the barometer standing at 30 inches, is employed as the standard of comparison for gases and vapours. One hundred cubic inches of air weigh, according to the latest researches by Regnault, 30·935 grains *at this temperature and pressure.* Air is therefore about 814 times lighter than water, as 100 cubic inches of water weigh 25246·0 grains.

"3. *If the specific gravity of a gas or vapour be known, the absolute weight of a given volume of it can be determined.* To accomplish this, we have simply to multiply the weight of an equal volume of air by the specific weight of the gas or vapour; the product will be the weight of the volume, at the standard temperature and pressure of the gas or vapour.

"Example.—What is the weight of 100 cubic inches of hydrogen, its specific gravity being ·0694?

"30·935 × ·0694 = 2·147 grains weight of 100 cub. in. of H.

"*If it be desired to find the volume of a given weight of any gas or vapour*, the weight of some volume (say a cubic inch) of the gas or vapour must first be ascertained by the preceding rule; then the given weight must be divided by the weight of the cubic inch; the quotient will be the volume in cubic inches of the given weight of the gas, at the standard temperature and pressure.

"Example.—What is the volume of 2·147 grains of hydrogen, its sp. gr. being 0·0694?

$$\frac{2{\cdot}147}{{\cdot}02147}=100 \text{ cubic inches.}$$

"EXERCISES.

"1. What is the weight of a cubic inch of oxygen, its sp. gr. being 1·1057?

"2. What is the weight of a cubic inch of nitrogen, its sp. gr. being 0·9713?

"3. What is the weight of a cubic inch of carbonic acid, its sp. gr. being 1·529?

"4. What is the weight of a cubic inch of gaseous ammonia (NH_3), its sp. gr. being 0·59?

"5. What is the volume of 54 grains of chlorine, its sp. gr. being 2·44?

"6. What is the volume of 45 grains of carbonic oxide, its sp. gr. being 0·967?

"7. How many cubic inches of oxygen would be obtained from 100 grains of chlorate of potash?

And in the Appendix II. this system is still further worked out; a series of questions originally given at the several examining boards is introduced, with references to such portions of the work as will enable the student to solve them. In all this a direct appeal is made to the exercise on his own part of the student's reasoning powers; and, at the same time, he has afforded him an amount of assistance not incompatible with a healthy development of his intellectual faculties. We need scarcely add that we most heartily wish Mr. Galloway's valuable and masterly treatise all the success it so honestly deserves.

As we have already intimated the two works, the titles of which head this article, though apparently on the same subject, materially differ in the object at which they aim. So far we have pointed out the distinctive features of Mr. Galloway's work; it now remains for us to notice those of Dr. Noad's. We confess that when we first saw it our impression was that in the presence of the existing

treatises upon the same subject (and here we, in an especial manner, allude to the masterly work on chemical analysis, by Fresenius), its appearance was uncalled for; but, upon examination, we found our first impressions to be wrong, and we gratefully acknowledge that Dr. Noad's chemical analysis will find a place for itself in our scientific literature. It is divided into twelve chapters, the first two of which are occupied with the consideration of what may be termed the prolegomena of chemical manipulation, the various apparatus and reagents required for qualitative and quantitative analysis being described in terse yet accurate terms. Chapter three is devoted to the chemical characters of the several metallic oxides; chapter four to those of the principal organic and inorganic acids; chapter five to those of the principal poisonous alkaloids; whilst chapter six is occupied with the consideration of the general principles upon which a qualitative analysis should be conducted. Chapters seven and eight are devoted to quantitative analysis; chapter nine to the analysis of organic bodies, chapter ten of mineral waters, chapter eleven of soils, and chapter twelve of the ashes of vegetable and animal substances. The work concludes with an Appendix containing several important tables, the first of which is of special value, inasmuch as it enables us to arrive at the exact amount of any elementary substance present in a given weight of any compound into which it enters. The application of this table will be understood by the following extract:—

"The application of this Table, and its convenience in quantitative analysis, may be illustrated by the following example:—

"Suppose as the result of an experiment to determine the amount of *sulphur* in a certain substance, 13·75 grains of sulphate of baryta have been obtained. On turning to the Table we find, under the head of "required *sulphur*" found, "*sulphate of baryta*," a series of figures arranged in nine columns, those in the first column indicating the amount of sulphur in a *unit* of sulphate of baryta, and those in the other columns the multiples of that number by 2, 3, 4, 5, 6, 7, and 9. In the case we are considering, we require to learn the amount of sulphur in 13·75 grains of sulphate of baryta: we turn to Column 1, where we find the number 0·13734; we put down this number, shifting the decimal point one figure further to the *right;* we next turn to column 3, where we find the number, 0·41202; we place this number under the former, preserving the decimal point unaltered; the next number is 7, under which column we find the number 0·96138; we place this number under the last, shifting the decimal point one figure further to the *left;* in like manner the

last number being 4, we write down the number found under that column, viz., 0·6870, shifting the decimal point two figures further to the left; the whole will consequently stand thus:—

"1·373400
0·412020
0·096138
0·006867

1·888425

13·75 grains of sulphate of baryta contain, therefore, 1·888 grains of sulphur."

So far we have only given our readers an insight into the matters to be found in this volume. It now remains for us to add, that whoever consults its pages will find in them full and clear directions for undertaking that most important duty, a chemical analysis. The work is evidently written up to the present stage of chemical knowledge, and is, in addition, enriched with well-drawn woodcuts. As already more than once stated these two works apply themselves to two widely different branches of the same science, and both merit our best commendation.

Register of the King and Queen's College of Physicians in Ireland; to which is appended a Roll of the Presidents and Fellows from 1654 *to* 1865—*Corrected to May* 1*st*, 1865.

We congratulate the College of Physicians on the appearance of this the most full and complete register ever published by them. It extends over 90 pages of large 8vo, and comprises a concise historical introduction, register of officers, of fellows, of honorary fellows, of licentiates, of licentiates in midwifery, as well as a general alphabetical register of the names, qualifications, and addresses of all ranks; and a roll of the presidents and fellows from the earliest period to the present time. A good deal of the historical introduction appears to be an abstract of a memoir of Dr. Stearne, which appeared in our last number; and much attention seems to have been given to furnishing the profession with what has been long wanted—a *resumè* of the existing legal enactments which concern the profession of physic in Ireland.

There is also in it a great deal of curious and almost unknown information respecting this college, which has on its roll many illustrious names.

As compared with other medical bodies, the corporation seems aristocratic, as it consists of forty-four fellows, eleven of whom are non-resident. Among the forty-four, however, are numbered our most eminent physicians. The government of this college is not virtually delegated to representatives, as in the cases of our more democratic corporations, for we read thus in the introduction, p. 11:—"The fellows alone constitute the corporation and govern the college, without the intervention of a board, council, committee, or senate."

The honorary fellows number thirty-three; the licentiates 597; and the licentiates in midwifery 170. Many of the licentiates are in all branches of the public service; and the college has numerous representatives on the continent, and in every part of the British dominions. An Irish physician may well desire to see his name in so honourable a roll.

From the appendix (roll of fellows, &c., 1654, 1865) we learn that the editor intends to extend it into short biographies. We suppose some of these will shortly follow the memoir of Dr. Stearne, the first fellow; but we trust it will be long ere the editor has an opportunity of giving us a biography of the last fellow—the Rev. Professor Haughton.

On Combined External and Internal Version. By J. Braxton Hicks, M.D., &c. London: Longmans. 1864. 8vo. pp. 72.

The object of this *brochure* is to recommend the general adoption of a mode of performing the operation of version, devised by the author, by introducing one or two fingers of the one hand, within the os uteri, and with them tilting the child in whatever direction it may be desired to turn it, and placing the other hand on the mother's abdomen, and with it assisting the movement of the child. This, as is pointed out by the author himself, is a combination of the method of external version of Wigand, with the method of Lee and others, who endeavour to perform the operation by introducing only two fingers into the os, and while it obviates the necessity for introducing the whole hand into the uterus, and

thereby lessens very considerably both the pain and risk of the operation, it facilitates its performance, and allows of its being done successfully where neither of the other modes would be practicable. Dr. Hicks claims for it that it permits of cephalic version being performed with almost as much certainty as podalic, but we must allow him to describe, in his own words, his mode of operating; and first as to *podalic version*, the head being the presenting part:—

"I will now proceed to describe the mode by which I effect podalic version. (See cases 1—16). We will suppose a case where everything is natural; the os uteri dilated to admit one or two fingers, membranes perfect, and the face towards the right side.

"The patient may be placed in the ordinary obstetric position.

"Having lubricated my left hand, I introduce it as far into the vagina as is necessary in order to reach a finger's length within the cervix—sometimes it requires the whole hand, sometimes three or four fingers will be sufficient in the vagina. Having clearly made out the head and its direction, whether to one side or the other of the os uteri, I place my right hand on the abdomen of the patient towards the fundus; I then endeavour to make out the breech, which is seldom a difficult matter. The external hand then presses gently but firmly the breech to the right side; as it recedes, so the hand follows it either by gentle palpitation, or by a kind of gliding movement over the integuments, while at the same time the other hand pushes up the head in the opposite direction, so as to raise it above the brim.

"It may here be mentioned that when the head has descended a considerable distance into the pelvic cavity or more than half way through the os uteri, it is scarcely possible to lift it above brim, especially if the uterus be active.

"When the breech has arrived at about the transverse diameter of uterus, the head will have cleared the brim, and the shoulder will be opposite the os. That is pushed on in like manner as the head, and after a little further depression of the breech from the outside, the knee touches the finger and can be hooked down by it. It very frequently happens when the membranes are perfect, that as soon as the shoulder is felt, the breech and foot come to the os in a moment, in consequence of the tendency of the uterus to bring the long axis of the child coincident with that of its own.

"Should it therefore be difficult to hook down the knee, depress the breech still more, and it will be almost always the case that the foot will be at hand.

"It will sometimes render turning more easy if, as soon as the head is above the brim, we pass the outside hand beneath it, and push it up

from the outside alternately with the depression of the breech. All this can generally be performed in a much less time than I have taken to describe it, although in some it requires gentle, firm, and steady perseverance, with such a supply of patience as is always demanded in obstetric operations."

In performing this operation ourselves, we have preferred to introduce the fingers of the *right* hand into the uterus, instead of the *left*, as directed by Dr. Hicks, because then, with the patient lying on her left side, the left hand can be placed on her abdomen without being carried up between the thighs, as should be done in Dr. Hicks' method, and this is not only less annoying to the patient, and less irksome to the operator, but allows him to have greater freedom of action.

In commencing the operation, the first step is of course to ascertain the position of the child in utero, so that it may be turned in the direction that will most easily accomplish the object in view.

"In regard to *transverse* presentations, it has already been pointed out (p. 8) that in their early stage it is really seldom necessary to employ more than one or two fingers within the os, in order to hook down the knee; should that, however, not be the case, then the best manner of proceeding, if we determine upon *podalic* version, will be simply to depress the breech from the exterior, as has already been described; if further assistance be required to produce that change, to push on the elbow as before mentioned, and the head up from the outside. This latter procedure is seldom required while the membranes are perfect.

"It may here be remarked that the use of the external hand in lifting the head from just above the pubes towards the fundus is very great, and tends to facilitate remarkably the operation of turning, even in those cases in which the old plan is used. In those cases where the descent of the feet and breech is found difficult, the head, easily recognised from the outside, can be pressed upwards if necessary to the very fundus. When this is accomplished the difficulty is generally over."

In cases of transverse presentation, where the head lies nearer the os than the breech, Wigand recommends cephalic version, by external manipulation, and we shall see that Dr. Hicks finds this operation of comparatively easy accomplishment by his method of combined external and internal action; where, however, the arm is prolapsed outside the vulva, and the thorax jammed into the cavity of the pelvis, while the head and breech are in the uterus, and the uterus tightly clamped round the body of the child, Dr.

Hicks thinks it advisable to introduce the hand into the uterus in the ordinary manner, but we presume he means this to apply to such of these cases only as the operation of version may be attempted in, instead of evisceration.

The following are Dr. Hicks' directions for *cephalic* version:—

"We will suppose first of all a case where the uterus is not active, the liquor amnii not escaped, or only recently so, where the fœtal hand has not passed the os.

"Introduce the left hand into vagina as in podalic version; place the right hand on the outside of abdomen, in order to make out the position of fœtus, and the direction of the head and feet. Should the shoulder, for instance, present, then push it with one or two fingers through the cervix in the direction of the feet. At the same time pressure by the outer hand should be exerted on the cephalic end of the child. This will bring down the head close to the os; then let the head be received upon the tips of the inside fingers.

"The head will play like a ball between the two hands; it will be under their command, and can be placed in almost any part at will. Let the head then be placed over the os, taking care to rectify any tendency to face presentation. It is as well, if the breech will not rise to the fundus readily after the head is fairly in the os, to withdraw the hand from the vagina, and with it press up the breech from the exterior. The hand which is retaining gently the head from the outside should continue there for some little time, till the pains have ensured the retention of the child in its new position, by the adaptation of the uterine walls to its form.

"Should the membranes be perfect it is advisable to rupture them as soon as the head is at the os uteri; during their flow and after, the head will move easily into its proper position.

"The ease with which cephalic version was effected by these means in one of the cases to be mentioned was such, that only half a minute was required. But if any unseen circumstance oppose difficulty to cephalic version, it will still remain open to us to adopt podalic in the same case. All that is required is to place the hand on the opposite end of the child, and depress it as has been already shown, pushing the head and shoulder from within; while the fingers are ready to hook down a knee or foot. This took place in one of the cases about to be recited. (Case 19.)

"From the opportunities I have had of judging I may lay it down as a good rule, that, in neck and transverse presentations, where we have no reason to hurry, and where all things seem otherwise favourable, we should first of all endeavour to induce cephalic presentation; and then,

should there be any difficulty in accomplishing it, to change our plans to podalic, which merely requires, as just shown, the action to be reversed.

"If in these malpresentations the fœtal hand protrude, it is even then quite possible to induce cephalic presentation, provided the thorax has not yet descended (as already mentioned. See Case 20). We must first, in this case, carefully replace the presenting hand into uterus, and then proceed as just indicated.

"The best mode of replacing the arm is first to bend the forearm upon the arm in the vagina; the fœtal hand will then be close to the breast of the fœtus; by pushing gently the elbow the hand will pass over the front of the chest and thus be effectually reduced, making the case much more simple.

"The advantages which accrue from the institution of cephalic presentation, in a case of labour without any other complication than that of malposition of the fœtus, is admitted by, I may say, all prudent practitioners, and the readiness with which it can in many such cases be effected by the above plan induces me strongly to recommend its adoption. Of course I mean with an ample pelvis, or small head, for in many cases transverse presentation is the result of malformed brim."

Dr. Hicks considers his method to be of special service in *malpresentations;* which may be rectified by it at an early period. In *convulsions;* where the uterus may be emptied of its contents, long before this could be safely done by other methods. In cases of *extreme depression;* where it is necessary to turn, this, he says, may be done with much less shock than when the whole hand has to be carried to the fundus. In *placenta previa;* where the os will not admit more than a finger or two, the bleeding may, he says, be controlled by laying hold of a leg, and drawing down the child as the os dilates, so as to keep it plugged, after the following method:—

"If I can feel the membranes and find the head presenting, I then proceed as in an ordinary case before described. When the foot has arrived at the os, I rupture the membranes, draw the foot and leg through as far as they can descend without force. By exerting gentle traction with merely the weight of the arm, we have a firm plug on the bleeding part. I retain hold of the leg in this manner; and thus as the os expands, the leg by its conical form makes an excellent plug, as does also the breech when it has entered the os.

"When this point is secured, most valuable time is at our command, and our endeavours should be devoted to restore the vital powers, weakened and in some cases almost annihilated by the previous loss. In

extreme cases—I am sure most of my readers will agree with me—the worth of this period can be scarcely over estimated.

"Labour pains are now waited for; and the case treated as is proper in breech and footling cases."

In Chapter V., Dr. Hicks summarizes the advantages and difficulties of his method of operating, and he concludes by giving cases illustrative of its performance in placenta previa, accidental hemorrhage, convulsions, coarctation of the brim, prolapse of the funis, and transverse presentations, in two of which latter cephalic version was performed.

In conclusion, we can only say, we have given Dr. Hicks' method a full trial, and we cordially endorse his recommendations of it.

Medical Errors. Fallacies Connected with the Application of the Inductive Method of Reasoning to the Science of Medicine. By A. W. BARCLAY, Cantab. and Edin., Fell. R. Coll. Phys.; Phys. to St. George's Hospital, &c., &c.

DR. BARCLAY read the Lumleian Lectures, for 1864, before the Royal College of Physicians of London; and it seems that the present volume embodies the lecturer's views; for we thus read in the Preface:—"The lectures are now published just as they were delivered, with the addition, only, of certain portions which were suppressed at the time, so as to occupy no more than the three hours allowed me."

Beyond this statement, and the announcement that the typography of this work extends over 123 pages of small 8vo, we regret we cannot give our readers any better description of the book as a whole; for Dr. Barclay has not thought proper to divide it into lectures, or chapters, or component parts of any kind; nor to give a table of contents, nor an index. This omission we hold to be a grievous defect in a book of any kind, but much more when the particular book contains lectures, and lectures on so very dry and uninviting a subject as medical logic. In the opening portion of this volume Dr. Barclay undertakes to show "that although much behind what are called the exact sciences, still the principles of medicine are largely based on true and legitimate inductions;" and he asserts "that all deductions ought to be based upon principles

first arrived at by the inductive method." He states that the first part of the process, in the construction of an argument based on the inductive method, consists in the collection of particular facts which agree in some one or more points; their harmony forming the groundwork of the inference which is afterwards drawn. The next step is to frame an hypothesis to link together, or to explain the phenomena; and this must, at least, possess the character of embodying the general fact observed in each of the particular instances. The final portion of the reasoning process is to apply to other cases, by way of inference, a law which has been already established by induction.

"An induction," he observes, "is only complete when it establishes a law of causation." To prove this he gives instances of induction, true and false. Vaccination he regards as an instance of true induction; and having described (p. 67) the process by which Jenner arrived at his conclusion; he adds:—"When, therefore, he proceeded to make the experiments of first inserting the vaccine virus, and producing the disorder known as cow-pox, and after a short interval inoculating the same individual with small-pox matter, and when he had observed that in such circumstances no effect was produced by the inoculation, his induction at once established a law by which future generations have been so greatly benefited. Occasional failures cannot overturn the force of his argument, though they may prove that in some individuals the protective influence is speedily exhausted, or that in others it only serves to modify the disease when it occurs."

The practice of syphilization, on the other hand, he affirms, has none of the elements of true induction. The hypothesis that it acts as a preventive, was not based on ascertained cases, and the theory that fresh infection eradicates an old taint, was opposed to all experience. The use of belladonna in scarlatina, Dr. Barclay also regards as an instance of false induction, and proves his case at such length as to preclude even a sketch of it here.

In a former number of this Journal[a] we had occasion to review Dr. Laycock's work on *Medical Observation and Research.* Dr. Barclay refers to the labours of Dr. Laycock, from whose views he largely dissents. He considers that Dr. Laycock "has unintentionally obscured what he designates 'the purely inductive method of research,' by importing into his consideration of it much

[a] May, 1864.

of the deductive, or *à priori*, system of reasoning. . . . . He explicitly states that the first step is the assumption of a theory, whereas the first real step is the ascertaining that we have facts to deal with, and a knowledge of their general bearing." To our mind Dr. Barclay is not only theoretically but practically right here. We all recollect that some years ago it was the orthodox theory, that there were certain conditions of disease, associated with what is called inflammatory action, in which blood-letting aided in the recovery of the patient. Whether this conclusion were right or wrong we shall not here discuss, but just remark that the assumptions made to give the character of a generalization to this supposed induction were of such a kind as to render conclusions drawn from them, whether true or false, quite untrustworthy. The blood drawn in such cases was observed to have the buffy coat, and hence it was concluded that blood-letting was useful because it withdrew from the circulation the morbid excess of fibrin. All this while it was forgotten that in withdrawing the fibrin most of the blood was also withdrawn, and the patient must needs die that the disease might be cured.

How many were thus bled to death none can say; but we all know that the reaction is now so great that scarcely any one dares to use a lancet, whatever he may think of the propriety of doing so.

Dr. Barclay draws a clear, but often ignored, distinction between medical statistics or averages, and true induction. This distinction may be thus shortly stated: an induction points out a relation of cause and effect, which can only be set aside by the interference of some more powerful and more universal cause; while an average, even when calculated on a large scale, is only a calculation of chances which gives no insight into the true relation subsisting between them.

It is scarcely necessary to show our readers that this is one of our most common medical fallacies; even in our most esteemed professional treatises numbers of cases are given without any indication of a direct relation of cause and effect. To prove this point Dr. Barclay instances statistics and inductions respecting rheumatic fever. Most persons recover from it, save when the heart becomes affected, and even then the deaths are comparatively few. We therefore learn almost nothing from the records of even a hundred cases of this disease. No matter what the treatment may have been been, and for all the selected cases show, the result might have been the same if nothing but cold water had been given.

Dr. Barclay discusses, at considerable length, the attempt recently made by the British Medical Association to obtain records of the results of treatment in certain specified diseases. He enlarges on Bouillaud's treatment of rheumatic fever by the copious and repeated abstraction of blood, and declares "it required no more than the evidence of his own cases to show that his application of it was most pernicious." On Dr. Hughes Bennett's connexion with this therapeutic inquiry he largely comments, and shows that if statistics be taken as the basis of medical argument the Edinburgh professor has not much to show; since it appears from the army statistics that 16,000 cases of pneumonia gave a death rate of only thirty-nine per thousand during twenty years, when moderate bleeding was the practice, while Dr. Bennett's death rate, as compared with this, is forty-three. The treatment of tape-worm by the oil of male fern, the treatment of non-syphilitic psoriasis, and the treatment of scarlatina are discussed as to their bearings on the question of statistical investigations and the method of averages.

To refer to these slightly in detail:—On the treatment of tape-worm by the oil of male fern, Dr. Barclay observes:—"No inductive argument is, in my opinion, more conclusive in the matter of therapeutics than this, viz., that when a tape worm is present in the alimentary canal, certain remedies, of which oil of male fern is one, affect the parasite so as to cause it to let go its hold, and this is followed in due time by its expulsion, and the cessation of the symptoms dependent on its presence. Half-a-dozen instances in which the effect distinctly and rapidly follows the cause, are just as good as a thousand for the purposes of an induction, which serves to establish a definite law of this kind."

Respecting the treatment of non-syphilitic psoriasis, he observes, that it holds a place somewhat intermediate between that of acute pneumonia and tape-worm, and says:—"While it does not present the same degree of vagueness as that which has reference to the treatment of acute pneumonia, it has less of certainty than the treatment of tape-worm, and less pretension to be ranked as an induction." The treatment of scarlatina, he considers, should stand on the same ground as that of acute pneumonia, "except that it is made only in regard to a small number of remedies, and therefore the series need not be quite so long in order to obtain trustworthy results."

Dr. Barclay shows, we think, that from a large number of case

trustworthy averages may be obtained, and that from these, under proper restrictions, correct inferences may, occasionally, be drawn; but that this does not give collections of cases the character of inductions. The distinctive character of an induction consists in the discovery of some law which will stand the test of experiment in every case, except where we can show its failure to be due to the interference of some higher law.

To define the term "law," as applied to medical research, occupies no small share of Dr. Barclay's attention. He defines it to be "an expression of the mode in which any given cause operates to produce a certain effect," and considers that a large proportion of medical fallacies arise out of a supposed application of the inductive method of reasoning. The processes by which a correct result may be arrived at, and errors avoided he does not enter into, although, in our opinion, he, perhaps, might have done so, instead of referring his readers to *Mill's System of Logic*, where in Book III., Chap. VIII., we may leave them—only wishing that they may "read, mark, learn, and inwardly digest;" and it will require all these processes to come to the bottom of so deep a subject.

We have already referred to Dr. Barclay's critical review of the medical fallacies contained in Bouillaud's treatment of acute rheumatism; we now proceed to note, *passim*, that he discusses the fallacy involved in the administration of sarracenia in small-pox, the administration of the hypopohosphites in phthisis pulmonalis, as well as the use of podophyllin and other remedial agents. One most important part we cannot lightly pass over. He looks on the late Dr. Todd's stimulating treatment of acute diseases as fallacious, because no experiments are given to show whether alcohol acted as a stimulus to the nervous system or as a general sustainer of life. Now we have looked over Dr. Todd's sixteenth lecture (in the large edition) "On the Therapeutical Action of Alcohol," and we think this statement of Dr. Barclay is scarcely correct. Dr. Todd does what Dr. Barclay does not—he defines the proper meaning of the term "stimulus," and the case numbered 84, in the same lecture, is most certainly an experiment of the most valuable kind. Had this work of Dr. Barclay's no other merit than that of directing attention to the absence of all connexion of cause and effect in the reasonings of many medical men we should gladly recommend its perusal to our brethren.

As a preliminary step, however, we think there is great occasion for avoiding two logical fallacies which we observe in several

lucubrations of our own day—the argument called *petitio principii*, or begging the question; and that so often appended to a record of treatment in a single case—*argumentum à particulari ad universale* "quod (say all the schoolmen) non valet."

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1. *Tabeller over Folkmængden i Christiania den* 31*te December*, 1863, *samt over de i Tidsrummet*, 1860–1863, *indtrufne Dödsfald.* Udgivne efter Foranstaltning af Christiania Sundheds-commission.

Tables of the Population of Christiania on the 31*st December*, 1863, *and of the Deaths which occurred during the period* 1860–1863. Published by direction of the Sanitary Committee of Christiania.

2. *Norges officielle Statistik. Udgiven i Aaret*, 1863. *C. No.* 4. *Beretning om Sundhedstilstanden og Medicinalforholdene i Norge i Aaret* 1861. Udgiven af Departementet for det Indre.

Official Statistics of Norway. Published in the year 1863. *C. No.* 4. *Report on the Sanitary Condition and Medical Arrangements in Norway for the Year* 1861. Published by the Department for the Interior.

3. *Norges officielle Statistik. Udgiven i Aaret* 1864. *C. No.* 5. *Tabeller over de Spedalske i Norge i Aaret* 1863, *samt Aarsberetning for samme Aar til Departementet for det Indre fra Overlægen for den Spedalske Sygdom.*

Official Statistics of Norway, published in the Year 1864. *C. No.* 5. *Tables of the Spedalskic in Norway in the Year* 1863, *and Annual Report, for the same Year, from the Physician-in-Chief for the Spedalskic Disease to the Department for the Interior.*

4. *Generalberetning fra Gaustad Sindssygeasyl for Aaret* 1856. Ved Ole Sandberg, Direktör. Christiania.

General Report of the Gaustad Asylum for the Insane, for the Year 1856. By Ole Sandberg, Director. (Similar reports are sent for the years 1858, 1859, 1860, 1862, and 1863.)

5. *Index Scholarum in Universitate Regia Fredericiana centesimo secundo ejus Semestri Anno MDCCCLXIV. ab A.D., XVII. Kalendas Februarias Habendarum.* Christiania, 1864.

6. *Index Scholarum in Universitate Regia Fredericiana centesimo tertio ejus Semestri Anno MDCCCLXIV. ab Augusto Mense Ineunte Habendarum.* Christiania, 1864.

7. *Om det syphilitiske Virus. Til Besvarelse af Priisopgaven: "Ere de Sygdomsformer, som vi hidtil have indbefattet under Benævnelsen Syphilis, frembragte ved* eet *Virus eller ved flere forskjellige?"* Af L. Bidenkap. (Belönnet med Hs. Mj. Kongens Guldmedaille.) Aftrykt fra *Norsk Magazin for Lægevidenskaben.* Christiania, 1863. 8vo., pp. 126.

On the Syphilitic Virus. In Answer to the Prize-question: "Are the morbid forms, which we have hitherto comprised under the denomination Syphilis, produced by a single *virus, or by many different poisons?"* By L. Bidenkap. (Awarded the Gold Medal of his Majesty the King.) Reprinted from the *Norwegian Magazine of Medical Science.*

8. *Iagttagelser under en Epidemie af Diphtheritis faucium.* Ved S. Höegh, Districtslæge i Vestre Söndmöre.

Observations during an Epidemic of Diphtheria. By S. Höegh, District Physician, &c.

9. *Det Kongelige Norske Frederiks Universitets Aarsberetning for Aaret* 1862. (Meddeelt of Universitetssecretairen.)

Annual Report of the Royal Norwegian Frederiks University, for the Year 1862. (Communicated by the Secretary to the University.)

The above catalogue represents a rather heterogeneous collection of works which we have just received from Norway. The space at our disposal will be insufficient to enable us to do more than give our readers, very briefly, a general idea of the contents of each publication.

1. From the *Tables of the Population of Christiania on the 31st December*, 1863, it would appear that this amounted to 55,125, of whom 27,142 were males, and 27,983 were females; 12,675, or 23 per cent. of the entire population, being under ten years of age, while 42,450, or 77 per cent., were above that age. The proportion over seventy years of age was 1·48 per cent. The mortality for 1863 was 23·32 per 1,000. The calculated mortality for the last five years was 25·59 per 1,000; but a remark is appended that the

city was during that period severely scourged by epidemic diseases, some of which contributed not a little to increase the mortality.

2. With the second "blue" book on our list we are saved some trouble by the announcement on the cover, that "a statistical *résume* is to be found at page iv." On turning to the latter we find, in fact, the following abstract in the French language, a translation of which will probably place our readers in possession of sufficient information respecting the subjects treated of in the volume:—

"During the year 1861 the general sanitary condition, and especially that of the two most northern dioceses of the kingdom, has been less favourable. Nevertheless the pathological constitution has not been of a very decided character. As to the epidemic diseases which have chiefly contributed to produce this unsatisfactory state, we would mention—the diseases of children, and particularly measles, which prevailed throughout almost the whole kingdom; diphtheria, which extended principally through the dioceses of Trondhjem and Tromsö, and through a great part of that of Christiania, and which must be considered as the most malignant disease of the year; as well as hooping cough, which likewise has been more than usually prevalent. Catarrhal and inflammatory diseases have also been a little more frequently met with than during the preceding years, while typhoid fever has kept pretty much within its ordinary limits.

"In 1861 the mortality has been rather greater than usual; calculated according to the estimated population of the year, or 1,635,617 individuals, it has amounted to 19·2 per thousand (not including the still-born; including the latter the mortality was 20·5 in 1,000). The corresponding numbers of the preceding year were 16·9 and 18·4. The number of deaths was 31,471; that of the still-born was 2,132. The number of births exceeded that of deaths by 18,053, being a diminution of 7,623 from the excess of the preceding year. Of 8,938 deaths, of which medical men have stated the causes, the following diseases have led to the greatest numbers:—Diphtheria and croup, 1,562; phthisis and pulmonary tubercles, 1,144; measles, 695; pneumonia, 551; typhoid fever, 506; decrepitude, 425; cerebral meningitis, 258; dropsy, 252; spédalskhed, 237; acute bronchitis, 220; hooping-cough, 217; cancerous diseases, 205; cerebral apoplexy, 186; convulsions, 159. In addition, 891 individuals perished by accident, the majority having lost their lives by drowning.

"The number of hospitals was 42; in the 34 ordinary hospitals 7,51 patients were under treatment, the mortality being 1 in 12·2. In seven asylums for the insane there were 794 inmates.

"The medical administration of the country was divided into 116 civil districts. The total number of authorised medical men was 331. Vaccination was performed upon 39,242 persons, whether by medical men or by assistant-vaccinators, the number of the latter being 635. There were 483 midwives who had passed their examination.

"The number of pharmacies was 54."

3. With respect to this volume also we need scarcely go beyond the summary given at page iii., that "the number of spedalskic individuals ascertained to exist in Norway, at the close of the year 1863, was 2,155, of whom 747 were in the hospitals."

4. The first annual Report of the Gaustad Asylum for the Insane, that, namely, for the year 1856, opens with a full description of the institution, illustrated with ground plans; and it and the reports of subsequent years contain much valuable information respecting the general care and medical treatment of the inmates. We must, however, content ourselves with quoting a few statistics from the latest of the reports, that issued for the year 1863. From it we find that the number of patients in the asylum during the year was 439, of whom 189, or 99 men and 90 women, were admitted in the course of the twelve months. A review of the monthly admissions during four years, taken together, would go to prove that June and July are the months most favourable to the development of insanity. The admissions stand thus (for four years):—June, 86; July, 77; December, 66; April, 65; March and November, 62 each; May, 61; September, 59; August, 58; February and October, 57 each; January, 51. It is curious that the number of those admitted between twenty and thirty, between thirty and forty, and between forty and fifty was the same for each decennial period, viz., 52. None was admitted under fifteen; one man and one woman were admitted between seventy and eighty. The patients were classed as labouring under—

Melancholia, - - -	75	males and	85	females	
Mania, - - -	53	„	50	„	
Stupiditas, - - -	4	„	1	„	
Delirium tremens, - -	1	„	0	„	
Dementia, - - -	74	„	56	„	
Idiotismus acquisitus, -	9	„	7	„	
Epilepsia, - - -	11	„	3	„	
Paralysis generalis, - -	9	„	1	„	

Of these the author considers the first four forms to be primary, and in general curable, and the latter four to be secondary, and, as a rule, incurable; hence 133 males and 136 females would, on admission, have been looked upon as curable; while 103 males and 67 females would, on the contrary, have given no hope of recovery.

5 and 6. The Indices Scholarum, being little more than programmes of the lectures to be delivered in the University, do not require further notice at our hands. We may, however, mention that the arrangements of the institution appear to be very complete, and that the facilities for instruction afforded by the several museums, laboratories, libraries, collections of instruments, and by the Botanic Garden, would seem to be very great.

7. The object of Dr. Bidenkap's work is sufficiently set forth in its title, which we have quoted. It would be quite impossible here to follow the author through his able review of the opinions of those writers who have preceded him, and through the numerous cases and experiments upon the examination of which he has based his own conclusions. The result to which his laborious investigation has led him may probably be sufficiently gleaned from a perusal of the last paragraph of his book:—

"To the views above laid down, respecting the relation between the two kinds of chancre, the author has, it will be found, been led by observations and experiments recently instituted in the section of the Royal Hospital for Diseases of the Skin. But Professor W. Boeck had already, chiefly from the results of the continued inoculations during syphilization, arrived at similar conclusions, namely, that the virus which produces the non-infecting chancre is identical with that which gives rise to the infecting, only that in the first case it possesses greater intensity, and is consequently limited to the production of a merely local phenomenon.[a] This view, it will be seen, very closely resembles that above put forward."

8. Dr. Höegh informs us that, between the 31st March, 1862, and the 4th May, 1863, he had under his care, in his district, containing a population of 8,700 individuals, 320 patients affected with diphtheria faucium, being 1 to every 27 of the population, or 3·6 per cent. Of these 320, 63 died, or 19·6 per cent., as follow:—

[a] W. Boeck, Recherches sur la Syphilis, p. 64.

Years.	Treated.	Died.	Per cent.
Under 1,	11	2	= 18
Between 1 and 5,	86	19	= 22
„ 5 and 10,	90	20	= 22
„ 10 and 15,	71	15	= 21
„ 15 and 20,	31	6	= 19
„ 20 and 30,	21	1	= 4·7
Over 30,	10	0	= 0

As to sex there were of the 320—

166 males, of whom 35 died = 21 per cent.; and
154 females, „ 28 died = 18 „

These tables speak for themselves, and are sufficiently interesting. As sequelæ of this malignant epidemic of diphtheria, the author notes:—

	Cases.
Paralysis of the throat (difficulty of swallowing, nasal voice), in	84
„ of the muscles of the nape of the neck, in	6
„ of the lower extremities, in	10
„ of the upper extremities, in	6
„ of nearly all the voluntary muscles, in	2
„ of the optic nerve, in	16
„ of the auditory nerve, in	12

Of other sequelæ there were:—

	Cases.
Long-continued weakness of all the vital functions (without paralysis), chiefly as a high degree of retardation of convalescence, in	10
Parotiditis, terminating in suppuration, with inflammation of the areolar tissue of the neck and formation of abscess, in	9
Furuncles, in	5
Exanthemata: erythema, vesicles, papulæ, and pustules, in	6

9. The Calendar of the University does not call for any special notice in this review.

Étude Clinique sur Divers Symptômes Spinaux Observés dans la Fièvre Typhoïde. Par le Dr. E. Fritz. Paris: A. Delahaye. 1864. Pp. 186.

The monograph by Dr. Fritz which now lies before us for review, and which had the honour of being crowned with a gold medal, presents us with the results of his clinical researches "Respecting certain Spinal Symptoms occurring in Typhoid Fever." Although the title of this work would seem to limit those symptoms to follicular fever, it may be necessary, at the outset, to give a caution, which the author curiously enough has postponed to the close of his book, that though they more especially belong to typhoid fever, yet this ailment does not enjoy a monopoly of them, as they may be equally found in exanthematous typhus, in double pneumonia, and other acute diseases. His observations were made at Paris in the wards of Drs. Barthez and Tardieu, of the hospitals Sainte Eugénie and Lariboisière, and occupied the years 1863 and 1864. Dr. Fritz undertakes the discussion of the question, whether in the classic treatises which have been written upon typhoid fever the spinal cord has had its due share of notice, and arrives at the conclusion that it has not. "In the portioning-out the nervous phenomena of this disease between the different parts of the cerebro-spinal system and the sympathetic, the spinal cord has found itself almost completely disinherited." He asks us confidently to name a single treatise which devotes a special chapter to the spinal symptoms of typhoid fever.

It must be acknowledged that though we find frequent allusions to those symptoms in works on fever and general medicine, and especially in those of Eisenmann and Wunderlich, of Germany, and of Lombard and Fauconnet, the distinguished physicians of Geneva, yet as a whole, the *rôle* which the spinal cord plays is as yet imperfectly understood. In these countries, more particularly, we have scarcely thought it necessary to discriminate between cerebral and spinal phenomena. We anticipate, therefore, that our author has, in his borings into a new mine, lighted upon a lode which is likely to afford a respectable produce. His merit is, that he found certain nervous symptoms, described as unusual, *bizarres*, exceptional, and that he traced successfully the connexion which attaches them to the general history of enteric fever. He discards the ancient axiom "rara non sunt artis," as applying more to symptoms viewed in an

isolated relation. If we take the general rule as the point of departure, and work synthetically downwards, we may fall into the error of regarding many natural deviations as monstrosities; whereas, if we work upwards, and carefully analyze apparent exceptions by the inductive method of Bacon and his followers, we shall take the best course to push the boundaries of knowledge some steps forward. Such is the course adopted by the author, and his "Table des Matières" consists of eight chapters, through which he perseveringly traces the so-named spinal symptoms in their manifold relations.

The first chapter of the book is dedicated to a historic description of the state of our knowledge upon this subject from the earliest records which we possess down to the period in which the writer took up the question. We think that some objections lie against the custom which prevails in medical books, of giving the bibliography of a disease *before* an account is given of the disease itself. The reader is likely to begin by taking ancient errors for truths which he is afterwards obliged to unlearn as errors. It is like conducting a stranger through the miry lanes and tortuous byeways of a city, before showing him its magnificent streets and architectural ornaments. We would, therefore, relegate the genealogical tree of the disease to the end of the book; or if that may seem a violation of the order of nature, we would admonish the reader generally to skip the historic chapter until he had read the subsequent ones. In a critical review, however, we may not find it convenient to adopt this course; but still the author's historic sketch is scarcely intelligible unless we keep steadily in view the conclusion at which he wishes us to arrive. By carefully comparing different parts of his book we think that it may be expressed in the following proposition:—

That typhoid fever with spinal symptoms is a disease totally different from cerebro-spinal meningitis, and that as yet we do not possess sufficiently satisfactory evidence that the latter disease can even complicate the former.

There are two cases given by Piorry (pages 131 and 137), which appear to confirm the possibility of the two diseases co-existing; also a case of Drs. Lombard and Fauconnet, entitled "Typhoid Fever Complicated with Symptoms which appear to depend upon an Acute Inflammation of the Spinal Marrow, or at least of the Membranes enveloping the Spinal Cord" (page 132); but the testimony of these cases the author endeavours to controvert

One of the earliest authorities upon spinal symptoms in fever

quoted in the sketch is Finke, who described an epidemic of bilious fever which occurred in 1776–1780, and particularly noticed the existence "of pains resembling rheumatism in the limbs and back, together with a prickling sensation in the muscles." We could cap his quotation with one of earlier date from one of our own authors, as follows:—"A pain in the small of the back, except in cases of small-pox and measles, is a general attendant on putrid fever, and may be regarded as a pathognomonical symptom" (Fordyce on Fever, 1774). It is a curious coincidence that Lombard and Fauconnet put forward the very argument of Fordyce, that the existence of pain of the back is a valuable means of diagnosis in doubtful cases of typhoid fever.

Coming nearer our own time, the first author, according to Dr. Fritz, who started the question of relationship between typhoid fever and spinal meningitis, was Dr. Grossheim, a German physician, in 1835. He dissected the spine in twenty-one cases, and arrived at the singular conclusion that abdominal typhus was *always* complicated with inflammation of the spinal cord, a conclusion justly pronounced by Grisolle as one which "n'a aucune espèce de fondement." The next attempt to explain those dorsal pains was to ascribe to them a rheumatic origin; and he comments on cases by Forget of Strasbourg, in 1841, and by Bazin, in 1834, of supposed rheumatic complications of typhoid fever. He examines Bazin's so-called arthritic form of typhoid, and discards it because the plaques of Peyer showed no signs of ulceration. He charges Forget with an error of diagnosis, that his rheumatismal variety is a misnomer, and that all the strange symptoms dominant in the case, such as rachialgia, general cutaneous hyperesthesia, &c., and which are wholly inconsistent with ordinary rheumatic pains, receive a perfect explanation upon the spinal theory. But by far the most important observations upon this subject belong to MM. Lombard and Fauconnet in 1843. In an epidemic at Geneva in 1840, these physicians noticed in some cases cervical pain radiating to the arms; also pain in the spine and lower extremities, and on consideration did not hesitate to refer those symptoms to a morbid condition of the spinal cord. "Since our attention was awakened on this subject," they observe, "we have questioned most of our patients, and have been truly astonished at the frequency of spinal symptoms of different degrees of intensity, and at different epochs of the malady." They mark particularly the following sets of symptoms, which we give in their own words:—

"1°. *La Céphalalgie Occipitale et la douleur Cervicale.*—Nous les avons observées assez souvent, pour pouvoir les considérer comme devant entrer dans la description générale de la fièvre typhoïde 2°. La douleur occupant les dernières vertébres cervicales et les premières dorsales s'accompagnant dans certains cas d'une grand gêne de la respiration; de douleurs suraiguës dans les flancs, a l'épigastre et dans les parties latérales du thorax; d'engourdissement de la màchoire et des bras. 3°. La douleur de la région dorso-lombaire de la moëlle épinière, à laquelle s'associaient des douleurs dans divers points de l'abdomen, quelquefois à l'épigastre, à la vessie, avec dysurie, incontinence ou rétention d'urine; enfin surtout la paralysie à divers degrés des extrémités inférieures."—P. 17.

We are reluctantly compelled to refer our readers for the continuation of the history of these symptoms to the book itself, and now proceed to give, very briefly, Dr. Fritz's "Exposé Analytique des Symptômes," in his second chapter, which occupies more than half the work.

The spinal symptoms are classed by him under two divisions; the first appertaining to the spinal cord in its entirety; the second having a distinct and special reference to the bulb.

The spinal symptoms, *properly so called*, comprise morbid disturbances, first, of the general sensibility of the cord, and secondly, of its motor functions. The general sensibility may be disturbed in three degrees; it may be abnormally increased, perverted, or diminished. As the author depends his case upon the first of those states, as that which carries with it "le cachêt le plus évident de son origine spinale," we shall confine our remarks chiefly to that condition.

The hyperesthesia of typhoid fever is composed, according to the author, of two elements, which habitually are combined in variable proportions; but one of them, at least, can exist independently of the other. Exalted sensibility may have its seat in the nerves of the external tegument, or in those which supply the muscles. A third form consists in an abnormal tenderness of the spinal apophyses, which, however, may be referred to the general hyperesthesia of the skin.

Cutaneous Hyperesthesia.—What are the distinguishing characteristics of this increased sensibility of the skin? As a general rule, which as yet holds good, it follows "une marche régulierèment

ascendante," and whenever a zone of the body becomes affected, it is to be found invariably in the parts situated beneath. As regards the parts engaged, the inferior extremities are most frequently affected. The next in the order of occurrence is the abdomen; in this it is usually diffused over the whole surface, but occasionally is concentrated in the iliac fossa; it is best perceived by pinching up lightly a fold of skin and pressing on it with the finger. After these in frequency come the thorax and back, then the superior extremities and the neck; but the increased sensibility always stops below the border of the lower jaw. The author gives cases intended to bear out these phenomena. We may remark that cutaneous hyperesthesia had been noticed by other observers before Dr. Fritz. We have before us a treatise on fever by Dr. Southwood Smith, published in 1830, from which we take the following extract:—

"It is not very common, but there sometimes takes place an extreme degree of tenderness over the entire surface of the body. The sensibility is so much increased that the patient cannot bear without pain the slightest pressure. Several cases have occurred in which the entire skin was as tender to the touch as the abdomen in some of the abdominal cases. Whenever this preternatural sensibility occurs, it is always in connexion with an exceedingly severe form of the disease" (page 173).

The same fact has been noticed by Dr. Graves as accompanying the epidemic of exanthematous typhus which occurred in 1834. (Clin. Lec., Vol. I., p. 231). Dr. Fritz accompanies the quotation, which he makes from the French translation by M. Jaccoud, which has already reached a second edition, with the observation—"It would be difficult to exaggerate the importance of the fact herein signified, *for it is Graves who speaks.*"

Spinal Hyperesthesia.—This form is more frequent but less regular than the cutaneous, and may exist in any part of the spinal column from the atlas to the sacrum. It is seldom met with throughout the whole extent; on the other hand Dr. Fritz has seen but one case wherein it was limited to a solitary spinal apophysis. It is carefully to be distinguished from rachialgia, with which it is often associated. In relative frequency of occurrence may be ranked the cervical vertebræ alone, the last cervical and first dorsal, and the dorsal. The test of its cutaneous character is this, that superficial pressure causes more pain than pressure firmly applied.

He found it to exist in various degrees in half his cases, and in more than half the cases of women and children. We must, however, not be contented with a verbal inquiry about it, as patients mostly reply to us in the negative; we must question the spine instead of the patient, and that in each successive vertebra.

After dwelling lightly upon muscular hyperesthesia, he discusses the various pains which patients refer to the back, all of which he includes under the general title of *rachialgia*. The subject is too extensive for us to enter on, especially as he connects it with radiations of pain to different parts of the body—with pains in the lower limbs, with pains in the chest, and with pains *en ceinture*. His description of perverted sensations is very brief, not occupying more than a dozen lines. He remarks on diminished and abolished sensation, of which he gives a curious case wherein a zone of natural sensibility interposed between two regions in which analgesia existed. We must also pass over the section upon the disturbance of the motor functions of the cord, though of much interest.

The most important part of the book in a practical application is that which describes those spinal symptoms whose origin is to be sought for in the medulla oblongata. They constitute a terrible array, a dark grouping in the general tableau of the disease. Among them are included sudden dyspnea without any thoracic affection, spasmodic cough, loss of the power of swallowing, speechlessness, spasms of the larynx, &c., &c. We would suggest to the author an expansion of this chapter in his second edition.

We pass from the chapters on the diagnosis, the etiology, and the prognosis of these spinal symptoms to the chapter on treatment, which is, in our opinion, the least satisfactory in the book. We do not attribute this to the fault of the author, but to the difficulty of the subject, namely, topical treatment in a general disease. In cases of severe pain of the back, which frictions with chloroform liniments are unable to relieve, he approves of cupping in preference to leeches. Bleeding he wholly discountenances. Blisters along the spine are more suited to the advanced stages, especially when loss of muscular power begins to be felt. Ice and all cold applications are particularly to be avoided in cases of grave spinal symptoms, such as spasm of the pharynx, &c., indicating disturbance of the bulb. For internal treatment he advises the use of purgatives; and thinks belladonna may be useful, while opium is dangerous. He makes no allusion to the use of mercury. As

according to his views typhoid is not complicated with spinal meningitis or myelitis, he declines entering into considerations upon the treatment of such an eventuality. He sums up with a useful caution respecting spinal cases, that the cord long retains a morbid sensibility which protracts convalescence. A good *résume* of the chief subjects discussed closes the work.

We rise from the perusal of this interesting book with a high opinion of its author as a man of originality of mind. He has not wholly satisfied us with his views respecting the impossibility of typhoid fever co-existing with positive inflammation of the membranes of the brain or cord, and terminating in the effusion of plastic lymph. We readily admit the almost universal absence of pus or exudation corpuscles in the brain or spine in typhoid fever, even in those cases where the spinal symptoms have reached "la violence la plus singulière," but the curious epidemics of cerebro-spinal meningitis, such as have been described by Dr. Mayne in this periodical in 1846, and by continental writers, require the matter to be further investigated. It is no little merit in this age, when trite facts are put forward as novelties, to have a class of symptoms brought out in their proper place, which were either imperfectly known or kept heretofore in the background, besides cataloguing them under their proper headings. We trust that his work is but the pioneer of many future discoveries in the same wide field.

The Optical Defects of the Eye, and their Consequences, Asthenopia and Strabismus. By John Zachariah Laurence, F.R.C.S., M.B., Lond.; Surgeon to the Ophthalmic Hospital, Southwark, &c., &c. 8vo, pp. 112. London: Hardwicke. 1865.

Ueber die Reihenfolge der Brillen-Brennweiten. Von Dr. A. Burow, Sen. 8vo, s. 20. Berlin: Peters. 1864.

On the Serial Order of the Focal Lengths of Spectacles. By Dr. A. Burow, Sen.

The great work of Professor Donders, recently published in an English dress by the New Sydenham Society, appears, at first sight, to render other treatises upon the same subject almost superfluous. To the busy practitioner, however, we feel that the study

of such a volume is almost impossible, and we have, therefore, made a very full analytical review of it, which we complete in our present issue; and Mr. Laurence, influenced by much the same feelings, has put together some of the chief facts connected with this branch of ophthalmology, and has formed from them a treatise which, not only contains a great deal of practical information in an accessible form, and will enable those who peruse it to diagnose and relieve the ordinary cases of impaired vision from optical defects, but which is also calculated to lead the student to desire a further acquaintance with the important subject of which it treats. Mr. Laurence has, in some sense, a claim to occupy the position which he has thus taken, since he was the first to make known in England, by papers published in the *Medical Times and Gazette* for 1860, the nature of the doctrines that Professor Donders was then teaching, and of the researches in which he was engaged

The treatise opens with a chapter on "Optical Considerations," which is chiefly devoted to a brief account of light, and of refraction and the formation of images by convex lenses. In this there is, of course, no room for anything new; but the author has accomplished his object with great clearness, simplicity, and brevity; and, especially in setting forth the nature of the relation between conjugate foci, with singular felicity of illustration. Mr. Laurence states in his Preface that for the last four or five years he has been daily engaged in the practical application of the principles that he explains, and his book thoroughly bears out the statement. It possesses the kind of clearness peculiar to a writer who has a working knowledge of his subject; and this clearness is the more remarkable since it owes little or nothing to the literary style of the descriptions of which it is a characteristic.

The second chapter deals with physiological optics—so far as it bears directly upon the main subject of the volume—that is to say, with the nature and mechanism of accommodation and of binocular vision. Here again Mr. Laurence is plain and intelligible, and has written just enough to meet the wants of a reader entirely unacquainted with the subject. We are disposed, however, to take exception to his account of the office of the iris in accommodation. His theory is that "the iris is an auxiliary organ for distinct vision—that the contraction of the pupil is intended as a corrective supplement to our accommodation, properly so called, by diminishing any slight circles of diffusion in the retinal image that might

possibly arise from inaccurate contraction of the ciliary muscle." It would, we conceive, be more correct to describe the office of the iris by saying that the contraction of the pupil, by cutting off the external and most divergent rays of a pencil, renders a smaller *amount* of accommodation effort necessary; and that it is an auxiliary destined to spare the labour of the ciliary muscle in degree, rather than to prevent confusion from its being imperfect in kind. On the subject of binocular vision Mr. Laurence contends, with reason, that the appearance of stereoscopic relief is due to a simple fusion of two perspectives, in opposition to the muscular sense hypothesis of Sir David Brewster. Like most other writers, Mr. Laurence speaks of the power of recognizing plane surfaces. His words imply the idea that a human infant, from the presence of certain appearances, finds out that certain things are solids, and have three dimensions. It is more probably true, from the vast preponderance of solids in the world, that the infant learns the occasional absence of the third dimension long after he is aware of its ordinary presence, and acquires the idea of a superficies at a time when the idea of a solid has already become perfectly familiar to him.

The third chapter, which is headed "Pathological Optics," contains only a definition of myopia and hypermetropia, and is followed by a fourth and fifth, which deal with these two conditions in their order. The methods of testing the state of the refraction, and of selecting the necessary spectacles, are clearly and concisely laid down. At p. 39, Mr. Laurence observes—"In testing each eye separately it is advisable for the surgeon simply to hold his hand in front of the eye not under examination; any compression of the eyeball by the patient's forcible closure of the eyelids may impede the free play of the eyeballs, and thus influence the refractive condition of the eye under examination." It is a better plan, we think, to add a small metal disk to the test case, and to fit this into the spectacle frame on the side corresponding to the eye not under examination.

The sixth chapter treats of astigmatism, and, from the comparative novelty of the subject, will probably be read with much attention. Mr. Laurence very clearly explains the nature of the affection, and gives some illustrative cases. His instructions for determining the kind and degree of astigmatism, and the character and strength of the glass required for its correction, are less explicit; but this, although technically an omission, is not one that need be regretted.

In truth, the examination of a case of astigmatism presents difficulties that can only be overcome by much experience. The surgeon is entirely dependent upon the statements made by the patient, who, on his part, is often inaccurate and confused. From this cause, as well as from varying states of the accommodation, different results are frequently obtained from successive trials; and, in order to explain and harmonize these differences, and to correct and allow for errors, a very complete and ready knowledge of all the factors of vision is absolutely required. For determining the state of the refraction in each meridian, as well as for ordinary cases of ametropia, Mr. Laurence recommends a simple and cheap optometer, made from a shoemaker's measure. This will do very well for a rough approximation, to be afterwards corrected by trials with lenses; but its graduation is not sufficiently fine to afford accurate results. By using a well-made instrument, in which the distance of the patient's cornea from the lens is fixed, and in which the distance of the test type from the optical centre of the lens can be read off by a Vernier scale to hundredths of an inch, spectacles may be ordered, for an intelligent patient, after one examination, with very tolerable certainty. Dr. Burow has pointed out that a star of thirty-six rays, placed in such an optometer, affords a ready means of determining astigmatism; and Mr. Laurence describes a method very recently introduced by Javal, who asserts that, in order to ascertain the most useful glass, both eyes should be tested together in the act of binocular vision. Nachet has made a binocular optometer for this purpose; but we have not as yet had any opportunity of determining its precise degree of utility.

Mr. Lawrence justly dwells upon the extreme importance of placing the axis of a cylindrical lens precisely in the right direction; and he figures and describes a useful little revolving instrument for measuring the angle which the axis should form with any chief meridian of the cornea. It will be found desirable, however, that the final adjustment of each lens should be made by the optician from absolute trial by the patient; and, in order to secure a good result, the caution given by Professor Donders about the perfect fitting of the frame should always be very carefully observed.

The seventh chapter is devoted to presbyopia. The discomfort arising from this condition has been much relieved by modern improvements in lighting; so that the presbyopic no longer discover their condition, as they did in Dr. Kitchener's time, by their tendency to bless continually the inventor of snuffers. Still, the

change which constitutes presbyopia being one that is common to all, and being, moreover, aggravated in the early stages of disease that imperatively demands treatment, this condition is, upon the whole, decidedly the most important of all those that require relief from spectacles. Mr. Laurence differs from Professor Donders in thinking that ten inches, rather than eight, should be held to be the distance of the near point necessary to constitute presbyopia; and he gives a short, but very clear, account of the nature of the defect, and of the means by which it may be remedied. The chapter concludes with a brief mention of the principle of Dr. Giraud Teulon's decentrated lenses; on the precise value of which Mr. Laurence declines, for the present, to express a positive opinion.

The three remaining chapters deal with paralysis of accommodation, asthenopia and strabismus. The first condition is especially likely to be overlooked; and Mr. Laurence has detailed some interesting illustrated cases, which will give the reader a very life-like conception of the disease. The chapter on asthenopia refers this condition—1, to optical defects of the eye; 2, to insufficiency of the internal recti; and, 3, to hyperesthesia of the retina; and gives instructions for the management of each. Under strabismus Mr. Laurence explains the manner in which this condition is brought about; describes an ingenious strabismometer of his own contrivance; and gives some statistics of the impairment of vision in strabismus, such as, we believe, have never before been published.

Upon the whole, we think that the purchasers of this unpretending treatise will find in it a guide that is at once concise, clear, and trustworthy; and that it contains sufficient information for the management of all ordinary cases of optical defect. It possesses the great merit of being practically original, even where it is not novel. Mr. Laurence has not compiled from German sources, but has worked at and assimilated his subject, and has then written down the principles and precepts on which his own experience has taught him to rely.

The pamphlet of Dr. Burow is intended to call attention to the great irregularity of the series of spectacle lenses contained in an ordinary test case; in which scarcely any two are separated by the same interval of difference. The difference between two lenses is of course to be expressed by the power of a third lens which, if added to the weaker, would make it equal to the stronger; and this power is found by dividing the sum of the focal lengths by their

difference. If we take F′ and F″ as the focal lengths of the two lenses, and F as the difference between them, then

$$\frac{1}{F} = \frac{1}{F'} - \frac{1}{F''}, \text{ and, therefore,}$$

$$F = \frac{F'.\ F''}{F'-F''}$$ Thus the

difference between a lens of 20-inch and a lens of 30-inch focal length would be a lens of 60-inch; which, added to the latter, would make it equal to the former; for

$$\frac{30.\ 20}{30-20} = \frac{600}{10} = 60.$$

By applying the same rule to one of Paetz and Flohr's ordinary test cases, we find between Nos. 2 and $2\frac{1}{2}$ a maximum difference equal to the power of a 10-inch lens; and between Nos. 36 and 40 a minimum difference equal to a 360-inch lens. In other words the maximum interval is thirty-six times as great as the minimum; and, moreover, with the exception of Nos. 20, 24, and 30, no two intervals are alike. From this defect Dr. Burow assigns two chief inconveniencies. In the first place some of the intervals are so much too large that certain eyes cannot be accurately fitted with spectacles at all; and, in the second, the cases in which the two eyes of the same person are of different powers, present peculiar difficulties.

For example, if a person requires for the right eye an 18 convex, and for the left eye a 10 convex, in order to read at ten inches, a time will come in which these glasses will require to be strengthened, on account of the increasing presbyopia of advancing life. The next number to 18 is 16, that is, an additional lens of 144; and the next to 10 is 9, or an additional lens of 90. The left eye will, therefore, receive a greater addition than the right, the near points of the two will no longer coincide, and binocular vision will be disturbed. Dr. Burow states, as the result of his optometric researches, that the refraction of the two eyes is somewhat different in nearly the half of mankind. Dr. Burow has found by experiment that, in complete paralysis of accommodation, there is still a certain small range of clear vision, due to the fact that the retina is insensitive to dispersion circles until they attain a certain magnitude. He finds that this range about equals a lens of 60 inches focal length; and he, therefore proposes such a lens as the fitting difference between successive lenses in the test case. To a series thus graduated he proposed to add a convex and concave of 120,

by which, in combination with others, the intervals of 60 can be reduced to 120, without multiplication of glasses. Test cases, fitted as described, are now supplied by Busch of Rathenow, and by Paetz and Flohr of Berlin; and, bearing in mind that the proposed improvement is only one of detail, it certainly possesses the merits of simplicity and convenience.

On the Anomalies of Accommodation and Refraction of the Eye; with a Preliminary Essay on Physiological Optics. By F. C. Donders, M.D., Professor of Physiology and Ophthalmology in the University of Utrecht. Translated from the Author's Manuscript by W. D. Moore, M.D., Dub., &c., &c. London. 1864.

(*Continued from Vol.* xxxix., *page* 152.)

We now proceed to briefly analyse the contents of the special part of this volume.

The fifth chapter treats of the emmetropic eye. We have already given (*vide* Vol. XXXIX., p. 140) the definition of this form of eye. It can be improved neither by convex nor concave glasses, and it can see accurately by means of its accommodation at relatively short distances. At one time it was thought that every eye was more or less myopic. But this is an error. In an absolutely mathematical sense no single eye is perhaps altogether emmetropic; but using this word in its ordinary sense, then, practically, the majority of the eyes of young persons is undoubtedly emmetropic. If it be asked, is this the most desirable condition, Professor Donders replies that, as far as regards himself, he would prefer a slight degree of myopia.

As in the translation of Donders' Memoir on Strabismus (*vide* Journal, Vol. XXXVII., 1864) the particulars of the centre of motion and movements of the eye, and the angle between the axis of the cornea and the visual line, were given in some detail, we shall no further refer to them here than to mention that in figures 4, 5, and 6, the axis of the cornea is *a*, *b*; and the visual line, or line drawn from the retinal image of the fovea centralis towards its object is *e*, *d*.

With the increase of years the eyes undergo a number of changes of different kinds. Some of these are at once recognizable: such

as the diminished lustre of the cornea and of the conjunctiva, the smaller pupil, the changes of colour and less transparency of the sclerotic and of the iris, the diminished depth of the anterior chamber of the eye, the arcus senilis, and others. Some are only found on anatomical examination, such as secondary changes of the retina; calcareous deposits in the posterior parts of the sclerotic; changes in the choroid; atrophy of the ciliary muscle; greater firmness and a yellower tint of the lens, &c. Even during life the comparative ophthalmoscopic examination of sound eyes at different periods shows that with the increase of years the perfect clearness and deep transparency, in virtue of which the fundus oculi of a child is seen with such incomparable clearness, are lost. With such changes different disturbances of function are combined—there is a diminution of the accuracy of vision, and there is a lessening of the range of accommodation. The cause of the diminution of the accuracy of vision depends, first, on there being less accurate images on the perceptive layer of the retina, and secondly, on the perception and conduction being more imperfect.

Presbyopia depends on a diminution of the range of accommodation, the far point remaining the same, but the nearest point of distant vision recedes yet more and more. This progressive removal of p (the near point) is a fact of universal experience. It is generally thought that this retrogression first makes its appearance about the fortieth year; but this is a mistake. Although it is generally about this time of life that the retrogression of p makes itself felt as a *disturbance* in the normal eye, yet, even in youth so early as previous to puberty, p moves considerably backwards. This change, it is well to recollect, affects all eyes equally—the myopic (provided it be healthy) as well as the hypermetropic and emmetropic eye.

If we enquire on what the change in the power of accommodation depends, the answer appears to be—On the lens. As one advances in life the lens gets firmer. In consequence of this the same action of the ciliary muscle can no longer produce the same change in the form of the lens. The slight diminution of refraction must also, it appears to Professor Donders, be sought for in the lens. Thomas Young remarked, and it has been more fully demonstrated by Senff, Listing, and others, that a lens with the laminated structure, and with its refractive power diminishing towards the periphery, has a shorter focal distance than a lens of similar form, but composed wholly of a substance of the refractive power proper to its nucleus, would have. Consequently if, with the advance of years, the

outer layers become more solid, a greater focal distance must be the result. Now, the existence of some such increase of solidity is evident from the increased reflection, in advanced life, on the anterior and posterior surfaces of the lens—a reflection which is proportional to the difference in refractive power between the outer layers of the lens and the aqueous or vitreous humor. This is capable, too, of being proved by anatomical investigation. In favour of this view is the fact, that the diminution of refraction goes hand in hand with the diminution of the power of accommodation, thus pointing to a common origin.

One sometimes hears the word "*far-sighted*" used as a synonyme for presbyopia; but it is incorrect, for the presbyopic eye is not necessarily more far-sighted than an emmetropic eye. It is well, then, to restrict the definition of presbyopic "to that *condition in which, as the result of the increase of years, the range of accommodation is diminished, and the vision of near objects is interfered with;* not forgetting, as we have said before, that it is the normal quality of the normal eye in advanced age—no more an anomaly than are grey hairs or a wrinkling of the skin.

It is not possible to fix a definite time for the appearance of presbyopia; or, at least, in doing so the boundary must be artificial. But there is a practical use in having even an arbitrary selected limit; and Professor Donders considers the commencement of presbyopia to take place when the binocular nearest point p, lies at about eight inches from the eye, though this does not in general involve the use of spectacles.

There may be, as we have seen, presbyopia of the myopic and hypermetropic, as well as of the emmetropic eye. The hypermetropic eye must be called presbyopic as soon as, in the use of glasses, which neutralize the hypermetropia, the binocular nearest point lies farther from the eye than eight inches. As to the myopic eye, keeping to the definition of presbyopia, this must commence in it when the distance at the nearest point of distant vision amounts to more than eight inches. Hence it follows that it is only to the slight degrees of myopia that presbyopia can belong—that with $M = \frac{1}{8}$ it is almost impossible. In slight degrees of myopia, presbyopia occurs much later than in the normal eye; and in this the advantage is by no means small. Not to require spectacles to see objects accurately until one's sixtieth or even seventieth year, is a very great privilege, and one that belongs to a myopic person, whose myopia equals $\frac{1}{10}$ to $\frac{1}{14}$ when the eye is not threatened with

any special danger. With slighter degrees of myopia a good deal of this privilege is still retained. A knowledge of these facts is often of great importance; thus, should we inquire into the hereditary nature of myopia, its existence in the parents is often denied; yet almost in the same breath it is added, as a proof of their excellent sight, that up to their fiftieth year, or even longer, they still read and wrote in the evening without spectacles, and we at once know what inference is to be drawn.

One of the most characteristic marks of presbyopia is the correction, by means of positive glasses, in the commencement of the effort, of diminished accuracy of vision of near objects. As soon as, by diminution of accommodation, in ordinary work, the required accuracy of vision begins to fail, there is need of convex glasses. An opinion very often held, is that the longer one refrains from using glasses, so much the better for the eyes, but it is nothing else save the purest folly to wear out the eyes unnecessarily, instead of, by wearing proper glasses, restoring them to their normal condition. Strangely enough, others have fallen into an opposite error, and believe that by the early use of spectacles, the power of vision will be preserved, and so they use or recommend "conservative spectacles;" both errors are most objectionable—the latter appears closely connected with principles of self interest. The following circumstances may modify the degree of the glasses required:—*a. A range of accommodation not corresponding to the time of life.* Those who are very much occupied in reading, writing, or other work requiring close attention, usually believe, when their eyes have suffered, that they have demanded very much from them; but comparative observations show that much close work does not necessarily injure the normal eye, and that the range of accommodation, in such people, diminishes not at all more rapidly than it does in agriculturists, sailors, and others, who for the most part look to distant objects.

Many morbid conditions, however, cause a change in the range of accommodation, such as general debility, the result of exhausting diseases—premature old age, glaucoma, commencing cataract, and the like. It is most important to remember that in threatening glaucoma, where common prudence requires us to avoid all tension of the eyes, the use of spectacles to enable the patient to read or work at near objects should be avoided.

b, Diminished Acuteness of Vision.—Where acuteness of vision fails, we must, so far as is practicable, rather meet the difficulty

with larger objects than with larger images, by abnormally diminished distances of the objects. Where there is chronic opacity of the cornea, or uncomplicated incipient cataract, or senile amblyopia, we must rest content with recommending the use of larger type, or occupation with coarser work.

c, The Nature of the Work to be Performed.—The work may be such that even the young normal eye is insufficient. Minute drawing, engraving, watch-making, minute anatomical dissection, require the use of the magnifying glass. But apart from its minuteness, the nature of the work sometimes requires a definite distance; in the use of certain musical instruments, for example, the distance of distant vision has to be from one and a-half to two feet. It will be thus seen what an amount of care is requisite in selecting proper glasses for all these different sorts of cases. It is something of a comfort to know that, though essential, injuries to sight are often, with much exaggeration, predicted from the undue use of convex glasses, yet Professor Donders has never, in the course of his large experience, met with any.

Hypermetropia, of which we knew so little until the researches of Professor Donders, is the subject of the sixth chapter. We have already seen that it depends on shortness of the antero-posterior axis of the eye (*vide* Fig. 3, Vol. XXXIX., p, 141). It may be either acquired or original. Original hypermetropia may be again divided into *absolute, relative, and facultative.* The *absolute* exists when, even with the strongest convergence of the visual lines accommodation for parallel or diverging rays is not to be attained. In youth it is of rare occurrence. The *relative* exists when only by the strongest efforts this accommodation can be attained. The *facultative* exists when parallel visual lines can be brought to a focus on the retina. With the first, unassisted vision never can be acute; with the second, only monocular vision, and this exceptionally, can be so; with the last, on the contrary, binocular vision may also be acute.

The hypermetropically-formed eye is generally a small eye, indeed it may be said to be an imperfectly developed one. This structure, too, is *hereditary*, and is very probably congenital. The diagnosis of hypermetropia is very easy, by the aid of glasses; but Professor Donders points out peculiarities of structure, not merely in the flat anterior surface of the sclerotic, the shallow position of the iris, the relatively small pupils, the apparently diverging strabismus, but he sees in the very form of the face the existence of this anomaly. The margins of the socket are flatter, less round;

the whole face is flattened, with little relief; often, too, the nose is but slightly prominent, and the upper part of its bridge is so little marked that it can scarcely give support to ordinary spectacles. The eyelids are flat and broad; the eyes are far from one another. If this peculiar physiognomy is found in a young person, though the external appearance of the eyes betrays no sign of disease, we may reasonably infer the existence of hypermetropia. To the question, "Can you continue to work long?" we obtain almost always the answer, "No." The employment of glasses will not always prove the presence of hypermetropia; it may be latent, and we must induce mydriasis, thereby destroying the powers of accommodation, and so rendering the hypermetropia manifest.

Asthenopia is intimately connected with hypermetropia, and it is a subject on which it is most important to have correct notions. We will find eyes apparently normal, and yet in reading or other close work, especially in a gloomy place or by artificial light, the objects after a short time become confused, and a feeling of tension and fatigue comes on, in and especially above the eyes. The person so affected now often inadvertently closes his eyes, and rubs his hands over the forehead and eyelids; after a little rest he once more sees distinctly, but the same phenomena are again developed more rapidly than before; the longer the rest has lasted, the better is the result. At first this state of things was considered as a sort of amblyopia, but by degrees the cause was sought more and more in the organs of accommodation; but now there can be little doubt that hypermetropia is usually at the bottom of asthenopia. In former years the very worst consequences were predicted from asthenopia. It was considered to be the first stage of, or at least to be intimately connected with, amblyopia; and this latter, it was supposed, threatened the destruction of the eye, unless perfect rest was given to the eye. But though Professor Donders has seen hundreds of asthenopic patients, who from youth up to their thirtieth or fortieth year, and some to a more advanced period, had every day of their lives, without or with too weak spectacles, pushed the tension of accommodation to the uttermost, yet he never saw any diminution of the acuteness of vision arise from this cause. Many were the forms of treatment proposed to cure asthenopia. Some, as Mackenzie, considered it often incurable. "Many a poor man," says this distinguished author, "have I told to give up his sedentary trade and drive a horse and cart, while to those in better circumstances I have recommended emigration, telling them that

though they could not use their eyes advantageously in reading or writing, they might do well enough to follow the pastoral pursuits of an Australian colonist." Had hypermetropia been known as the cause of asthenopia, so very decided a step in the life of a man need, perhaps, not have been taken. But the prejudice against using sufficiently convex glasses was deeply rooted, for the same writer says:—"A child, the subject of asthenopia, engaged in learning his lessons, complains he cannot see, and repeats the complaint so frequently, especially by candlelight, that his father or grandfather at last says, 'Try my glasses.' *The child now sees perfectly*, and night after night the loan of the glasses is required before his task is finished." But instead of recommending the use of these glasses, he adds:—"It would have been better had glasses been selected at the longest focus that would have enabled the child to read, or *better still* that he had been put to bed, and the lesson left until daylight." Now asthenopia need no longer be an inconvenience to any one, for the discovery before alluded to pointed out the way to obviate it. This subject of hypermetropia, and its relations to asthenopia, is one of the most important in this very able work. Would space permit, we would gladly condense this whole chapter; but the connexion of hypermetropia with converging strabismus has already been fully discussed in our pages (Vol. XXXVII., p. 480), and we will only now subjoin the following most interesting supplementary cases. These illustrative cases are well worthy of a careful study:—

"We learn to distinguish, at first sight, an ordinary case of asthenopia, the result of H.

"II. Miss H., aged 19, is announced. She has a florid look, has clear eyes, without a trace of disease, blue iris, mobile pupil, not a very deep globe, flat margins to the socket, the visual axes appear to diverge. I suspect asthenopia. I make her read, and bring the book to 6″: reading becomes difficult; at 5″ it is impossible. There is either H. or diminished $\frac{1}{A.}$ My eye falls on those about her, I see a brother with converging strabismus. This was decisive in favour of H. 'You cannot persevere with your work.' She answers: 'No.' 'On exertion you get a feeling of tension over the eyes, press upon the part with the hand, rub over the closed eyes, and then it passes off, but only for a short time?' 'Precisely,' is the answer. Confidence is gained. 'You have no pain in the eyes?' 'At a distance you see well?' 'Yes.' 'After a long rest you can continue your work better?' 'Yes, yes.'—With $\frac{1}{18}$ she distin-

guishes well at a distance, and moving the glasses at the first moment not so well; with $\frac{1}{16}$ not so acutely as with $\frac{1}{18}$, with $\frac{1}{24}$ not more acutely; between the two eyes there is little difference. Ophthalmoscopically all is well. I learn further, that for some years the inconvenience felt in working has been always increasing; that formerly when weakened by fever, she could for a time neither read nor sew; that she once tried a pair of spectacles, but was strongly cautioned against wearing them, &c., &c. She gets spectacles of $\frac{1}{16}$ to work with, with a recommendation now and then to pause for a little, and at first not to do much in the evening. At the end of a week she has forgotten her ailment. She now works too with less trouble occasionally for a short time without spectacles, which I advise her to do, though with the recommendation to return to the use of the spectacles on the least trace, or rather before the occurrence of fatigue.

"Asthenopics have sometimes a sad past, and live in a gloomy future.

"III. The Rev. G. D., aged 52, looks dejected. 'My good Professor,' he says, 'I come to you, for I feel that I am getting blind!' For the last twenty years he has thought that within a year he should be blind; and, singularly enough, although he still sees, he continues to look upon every year as the last! Such is the man! His life has been a struggle with his eyes. Even as a child he read with difficulty. When a student, the least exertion fatigued him, and he was compelled to learn more by hearing than by his own study. As a preacher, he has been obliged to write his sermons in a rather large hand, and still to get them off by heart. And, what was the worst part of it, he never read nor worked without the idea that he was thus hastening his blindness—interfering with the concentration of his mind upon any definite object. The same fear of blindness had restrained him from a matrimonial alliance with which he believed his happiness for life to be connected. He trusted in art. He had faith in a person he consulted in Germany; and if the optician had sometimes given him spectacles which had brought him relief, these were mercilessly taken from him again by the oculist on the first consultation, as a treacherous instrument which must, in the end, inflict upon him the total loss of his sight. At last he had, in his fortieth year, got convex glasses of $\frac{1}{40}$, and he now uses $\frac{1}{20}$. 'Do you see with these spectacles at a distance?' was my first inquiry. 'Something better,' he replied, 'but still very imperfectly.' I tried $\frac{1}{10}$: 'Much better,' was his verdict;—subsequently I gave him $\frac{1}{8}$: 'Still better.' In

a word there was $H = \frac{1}{7}$ with $S = \frac{17}{20}$, and, with his slight range of accommodation, he needed glasses of $\frac{1}{5 \cdot 5}$, in order to make reading at the distance of a foot easy. He got $\frac{1}{7}$ to wear. The man was grateful as a child. He left me as one saved from destruction.—Such victims of the prejudice against the use of convex glasses are not uncommon.

"Where H exists, paralysis of accommodation may give rise to disquieting symptoms.

"IV. E. K., a boy of ten years, son of Dr. K., remarks in the morning that he is not in a condition to read. His father sees that the pupils are rather large and are immovable. Paralysis of accommodation occurs to him; but at a distance also, the boy cannot properly distinguish objects: 'there must consequently be a lurking affection of the optic nerve or of the brain.' He brings his son to me. I establish the fact of paralysis of the sphincter of the pupil in both eyes. Neither on convergence, nor on the incidence of strong light, does contraction of the pupil arise: accommodative and reflex movements are both absent. The inference that there is paralysis of accommodation is thus justified. Why cannot the boy see even at a distance? A glance with the ophthalmoscope clears all up: it appears I must accommodate about $\frac{1}{12}$ in order to see in the uninverted image the fundus oculi; and, as I am emmetropic, our boy has therefore H of about $\frac{1}{12}$. With $\frac{1}{12}$ he then saw admirably at a distance; with $\frac{1}{6}$ he read at the distance of a foot. All fear of an affection of the system of the optic nerve was gone. In speaking of the anomalies of accommodation, I shall revert to such cases. Here it may suffice to observe that within four weeks the paralysis had given way, the H had again gradually become for the most part latent, and that in what the boy had to do or to read, he now no longer complained even of fatigue. In a few years asthenopia may be expected, and the use of the convex spectacles while working will then be indicated."

The subject of aphakia concludes this chapter. The absence of the lens in the dioptric system of the eye is in many respects an important condition. It is therefore strange that it had not even received a name until Professor Donders called this condition by the name of aphakia. It occurs either as the result of operation for cataract, or of a wound, and sometimes spontaneously. From the discussion of this subject, we learn that if the eye was myopic before the development of cataract, weaker glasses are required

after the operation than if the eye had been normal. In aphakia a small pupil is of great advantage; therefore, when iridectomy is performed, in connexion with the extraction of the cataract, it ought to be performed superiorly. In this case the part which has suffered excision is almost completely covered by the upper eyelid.

As was evident from the facts mentioned above (Vol. XXXIX., p. 135), not the slightest trace of accommodation exists in aphakia. In old persons with imperfect acuteness of vision, one is apt to fancy that there is a certain amount of range in accommodiation; but in young persons, in whom one might reasonably expect to find more accommodative power, it is evident on examination that it is entirely lost. Here, therefore, the accommodative power must be supplemented by the hand, by which the distance between the glass and the eye can be altered. The lens placed before the eye has been substituted for the crystalline. It can also take on itself the part of the accommodation. It cannot do this by altering its form to the lens in the eye, but it holds to the old theory, according to which the power of accommodation was made to depend on the displacement of the lens. Notwithstanding this artificial power of accommodation it is in general advisable in aphakia to use two pair of spectacles, one for distant, the other for close vision.

The Seventh Chapter treats of myopia.

We have already seen that in this condition of the eye the focus of the dioptric system lies in front of the retina (*vide* Fig. 2, p. 141, Vol. XXXIX), or that parallel rays derived from infinitely remote objects unite in the myopic eye in front of the retina. In general it is easily recognized—near objects are very distinctly seen; while at a distance vision is, on the contrary, diffuse. When letters of double magnitude are not seen at double the distance, we are in general safe in assuming the existence of myopia. We determine its degree by trying what the *weakest negative glass* is, with which vision is as acute as possible; and next to the examination with glasses is that with the ophthalmoscope. Position in society appears to have a great influence on the distribution of myopia, those in easy circumstances being much more afflicted with it. It is not, however, equally prevalent in all countries, but appears to be more specially proper to cultivated nations. It is very prevalent in Germany. The distribution of myopia indicates tension of the eyes for near objects, as its principal cause. But the explanation of this fact is not so very evident. Myopia depends on a prolongation of the visual axis, and this is not altered in accommodation for near objects

How, then, is this prolongation to be explained? First, by pressure of the muscles on the eyeball, in strong convergence of the visual axis; second, increased pressure of the fluids, resulting from accumulation of blood in the eyes in the stooping position; and third, congestive processes in the fundus oculi, which, leading to softening, gives rise to extension of the membranes. In connexion with these causes, the injurious effect of fine work is, by imperfect illumination, still more increased. A stooping position of the head in reading or writing is also injurions. To this is to be ascribed the fact, that in boarding schools where the pupils by bad light read small print in the evening, or write with pale ink, the foundation for myopia is oftentimes laid. While with watchmakers and other such artizans, although they sit all day long with a magnifying glass in one eye, we observe no development of myopia, undoubtedly because they fix their work only with one eye, and therefore converge but little; they, too, generally avoid a stooping position.

It is a very common and sad error, that the near-sighted eye is considered as particularly strong, and it is one that is believed in even by medical men, who surely ought to know better. But there need be no hesitation in asserting that the myopic eye is *not* a sound eye. In it there exists more than a simple anomaly of refraction—there exists a marked extension of the membranes, and if this has proceeded to a certain limit, it cannot remain stationary, and so progressive extension includes progressive myopia, which is a serious disease of the eye. In youth almost every myopia is progressive, and this is often continued with symptoms of irritation. This is the critical period for the myopic eye. If it does not increase too much it may become stationary, and may even decrease in advanced age. If it is developed in a high degree, it is difficult to set bounds to it, hence at this period it needs every care, and on this too much stress cannot be laid. If it continues progressive, the eye will soon, with troublesome symptoms, become less available; and it not unfrequently happens, that about fifty or sixty years of age, the power of vision, from separation of the retina, effusion of blood, or from atrophy and degeneration of the yellow spot, becomes irretrievably lost.

Myopia may be divided into *stationary*, *temporarily progressive*, and *permanently progressive*. High degrees of myopia never appear to be congenital, and it rarely arises after the fifteenth, and never after the twentieth year, in eyes that were previously normal. When this is supposed to be the case, it will be found that the primitive

slight degree of myopia was overlooked. It would appear that fundamentally emmetropic eyes seldom, fundamentally hypermetropic eyes perhaps never, become myopic; but that having once occurred a tendency to myopia is transmitted to posterity, and, under fresh exciting causes, is developed to its higher degrees. It is not necessary to detain the reader with all the ophthalmoscopic appearances of myopia. The principal changes are, atrophy of the choroid, or of the outside of the entrance of the optic nerve; and this, when highly developed, is combined with a change of form of the nerve surface, a straightened course of the vessels of the retina, incomplete diffused atrophy in other parts of the choroid, and morbid changes in the region of the yellow spot. All these are particularly alluded to and described in this volume. In reference to the question of the connexion of staphyloma posticum with inflammation, Professor Donders concludes, "that almost without exception the predisposition to staphyloma posticum exists at birth; that it is developed with symptoms of irritation, which in moderate degrees of staphyloma do not attain any great clinical importance; but that in the higher degrees an inflammatory state almost always occurs, at least at a somewhat more advanced time of life, as a result, and as a co-operative cause of the further development of the distention, and of the atrophy. The predisposing cause of the staphyloma would appear to be arrest of development."

In higher degrees of myopia there is a tendency to approach closer than is necessary to the objects, and this particularly at sedentary work, and so to assume a stooping position. With increasing myopia, too, the fancy increases of occupying oneself with small objects. Persons so affected prefer small type, accustom themselves to small hand-writing, and avoid long lines—by so doing of course they see more at once. Myopes accustom themselves, too, to approximate their eyelids so closely that it actually belongs to their physiognomy; those affected with it seldom fix the persons to whom they speak, because they but imperfectly distinguish their features. In general they have no correct idea of the impression which their person and their mind make upon one another; and according to their original disposition, a peculiar freeness and too great self-confidence—or else what is rarer, a more than ordinary bashfulness, are thence not unfrequently developed. In their bearing, and often also in their gait, a certain awkwardness is frequently manifested, by which some are recognized at a distance. Finally, much more of what passes in the world escapes them than they

themselves are aware of, and with respect to a number of things their knowledge is less correct, because they fill up what is deficient from their own imagination. Cardarus even says that myopes are particularly amorous, because that not observing defects, they look upon human beings as angels. The starry heavens they also see in extraordinary splendour.

Besides diminished acuteness of vision, myopes often suffer from the rapid supervention of fatigue, a feeling of tension in the eye, sometimes pain or pressure, and also from photopsia (false perception of light) and muscæ volitantes. These two last phenomena often constitute a permanent source of complaint, even when no particular state of irritation and no extraordinary diminution of the sharpness of vision exist. Ordinary muscæ volitantes are not attended with any essential inconvenience. Myopes are, however, troubled with them perhaps more than others, and with them they increase and become less transparent in advanced life. Photopsia generally accompanies the more acute amblyopic symptoms of myopia; sometimes it is constantly present, and will yield to no treatment. This phenomena must, it is feared, be regarded as a bad omen, and as a sign of diffused atrophy.

In the translation of Donders' Memoir,[a] the connexion of myopia with insufficiency of the internal muscles, and with diverging strabismus, has already been given, so that we need not again refer to this portion of the subject.

The cure of myopia belongs to the *pia vota*, but the task of the ophthalmic surgeon resolves itself into the following:—

1. To prevent the further development of the myopia, and the occurrence of secondary disturbances.

2. By means of suitable glasses to render the use of the myopic eye easier and safer.

3. To remove muscular asthenopia, by the use of glasses, or tenotomy.

4. To combat the secondary disturbances of the myopia.

We would that space permitted us to go into each of these subjects in detail; but we can at most only allude to the chief points, imploring the reader who may have to deal with this subject to read the *ipsissima verba* of the volume itself. I. Strong convergence must be avoided; the patient should look much at a distance; for near objects he should have spectacles, which bring the farthest

[a] L. c. p. 480.

point of distinct vision to a sufficient distance—say from sixteen to eighteen inches. Work should—every now and then—every half hour—be discontinued for a couple of minutes. A stooping position must be avoided with the greatest care; the height of the desk on which the book or paper rests should suit the height of the head, and the inclination should be as great as circumstances permit. In writing, the limits are, when the ink no longer flows from the pen; but even then a pencil may be used. Further, those who are highly myopic must be earnestly dissuaded from everything that gives rise to increased action of the heart, and to tendency of blood to the head, with a view both to limit the progress of the myopia and to prevent the occurrence of secondary affections.

II. The prescribing of spectacles for myopes is a matter of great importance; unsuitable glasses are here most dangerous. In hypermetropia we must be aware of giving too weak, but in myopia of giving too strong glasses. In some cases the myopia may be neutralized—such, for example, as when the glasses are used exclusively for distant vision, and when the myopia is slight in reference to the range of accommodation and the eye is otherwise healthy. But *many circumstances forbid us to completely neutralize the myopia ;*—they are connected with. *a*, The degree of the myopia ; when this equals $\frac{1}{3}$ and upwards perfect neutralization is inconvenient, owing to the smallness of the images formed. *b*, The range of accommodation; where the patient is past five-and-thirty and glasses have not been used from youth the tension required to see near objects with properly neutralizing glasses is too great to be maintained. *c*, Acuteness of vision; when this is diminished there is great danger in giving strong glasses. *d*, Age; the influence of age is, for the most part, comprised in the diminution of the range of accommodation and of the acuteness of vision. *e*, The nature of the work and the distance at which it is to be performed; when the myopia is properly neutralized in youth and if the sight be otherwise good, the eyes will be able to perform any ordinary work at ordinary distances; but, when this has not been done, the object will be to bring the farthest point to that precise distance at which acute vision is requisite. Thus, for example, for ladies, in the reading of music, the far point must be brought to within eighteen to twenty-four inches; so, also, for seeing manuscripts on the lecture table or in the pulpit.

The first incorrect estimate of magnitude, distance, and form is very quickly lost on wearing spectacles; and it is remarkable how

the eyes, being accustomed to look nearly through the axes of the glasses gradually limit their movements; even on the removal of the glasses this limitation continues, and the required movement is obtained by turning the head, thus giving a peculiar bearing to myopic people accustomed to wear glasses.

III. In high degrees of myopia there is often insufficiency of the internal recti; this may be developed in different degrees, from relative to absolute diverging strabismus; this first makes itself known by muscular asthenopia in binocular vision for near objects. It is often very difficult to know what to do in such cases. In non-myopes, suffering from insufficiency of the internal recti, systematic practice with prismatic glasses is of the greatest service; but here this must not be thought of. Von Gräefe even decides that in cases of excessive myopia it is better to submit to the tendency to divergence; the chief remedy of all is tenotomy of the external rectus; even where myopia is the cause of the affection it may sometimes yield excellent results. The condition, *sine qua non*, for tenotomy is, that under the attempt at single vision a sufficient divergence of the visual lines should appear to be possible; but for a full discussion of this subject we must refer our readers to Von Gräefe's paper in the *Archiv für Ophthalmologie* or Giraud-Teulon's work *Sur le Strabismé.*

IV. Therapeutic treatment. For myopia, as such, there is no therapeutic treatment—myopia consisting in an anomaly of form capable of no improvement. Hygienic measures will, undoubtedly, often check its further development, but it is not unfrequently complicated with symptoms of irritation and inflammation, and with other pathological deviations of different kinds which partly proceed from, partly promote its further development; and with respect to these it is the duty of the therapeutist to the best of his ability to interfere; with a few hints as to therapeutic treatment this chapter closes.

The eighth chapter treats of astigmatism; this is divided into *a*, irregular astigmatism, where there is an aberration which has reference to the rays refracted in one and the same meridian; *b*, regular astigmatism, where there is an aberration dependent on the focal length of the different meridians of the light refracting system. The cause of regular astigmatism is to be partly sought for in the cornea; and measurements have shown that the cornea in its several meridians has a different radius of curvature, whereas irregular astigmatism depends on the lens, so that in aphakia it is absent,

however the lens also modifies in a degree irregular astigmatism. So long as astigmatism does not essentially diminish the acuteness of vision it may be called normal; if it amounts to $\frac{1}{40}$ or more it must be considered as abnormal.

The peculiar phenomena attending astigmatism are given in minute detail, but our space will not allow us to do more than thus allude to them; of the methods used for its diagnosis we find those of Professor Donders, of Young, Airy, and of Stokes, the well-known professor at Cambridge. General rules are laid down for the employment of cylindrical glasses. Astigmatism would appear to be either congenital or acquired, but it is in most cases congenital—one of the parents labouring under the same defect, but it often happens that different children, born of the same parents, exhibit this anomaly. In the majority of cases both eyes are affected; often, however, one is completely or almost completely free. When a high degree of ametropia exists on one side, asymmetry of the bones surrounding the orbit is a very common phenomenon. Professor Donders has met with astigmatism more frequently in men than in women, but this may be accidental. Although time of life can have no influence on astigmatism yet the disturbance of vision from this cause is less when the power of accommodation remains active. Of congenital regular astigmatism of the cornea we have myopic, and hypermetropic astigmatism, simple and compound of both, and mixed astigmatism; of acquired regular astigmatism we find that "depending on the cornea" and that "seated in the lens." Irregular astigmatism may, as well as the regular, be divided into normal and abnormal—the latter depending upon irregularities of the cornea as well as upon those of the lens; to it may be referred conical cornea, spots on the cornea, and some of the acute affections of the cornea, with transparent ulcers—the former depending on the structure of the lens, and these, too, as suggested by Bowman, occurring sometimes independent of cataract and sometimes attending its earlier stages. This chapter on astigmatism is one of the most important in this volume, and we regret very much that want of space will not permit us to append several of the accounts of cases.

Chapter nine concludes the special part, and treats of the difference of refraction in the two eyes; the statement is often made that the right and left eye usually differ considerably; but this is a mistake; like the organs of animal life in general the eyes present a great symmetry between the right and left side.

The second, and by far the shortest portion of this work, is devoted to the consideration of the "accommodation." Although differing from the "anomalies of refraction," inasmuch as that the latter are anomalies of the form and the former disturbances in the functions of muscles, the connexion between the two is too obvious to be overlooked. Accommodation depends on muscular action; its anomalies are proper to muscles in general; paralysis and spasm; in connexion with the former the action of mydriatics, in connexion with the latter that of myotics is to be studied. These subjects are treated of in three chapters. The first is on the influence of the nerves upon accommodation and upon the movements of the iris; the second is on paralysis and debility of accommodation; this chapter contains an important section on the paresis of accommodation after diphtheria; the third is on spasm of accommodation, and treats fully of the subject of the Calabar bean; this chapter concludes this volume of which we have thus given a very brief analysis, one that may just point out to the reader the place where he will be sure of finding all that is known on the subject. We have preferred to make this notice more analytical than critical. In doing so we know that we have selected the more difficult task, for the best book printed is never free from many errors, the pointing out of which is never an arduous undertaking. To condense an author's statements, and yet not to misrepresent them, is a very different matter; our only hope is, that in trying to do so here we may not have altogether failed. A well-known ophthalmic surgeon has lately stated, that as well might Great Britain still claim the United States of America as part of her kingdom, as general surgery assert that ophthalmology is nothing but a small portion of her own domain.[a] Such works as this of Donders' will go far to prove this to be a correct statement. What general surgeon will care to master all the intricacies of this volume and when will general surgery itself claim the same rank among the exact sciences which ophthalmic surgery now does; but be this as it may, one thing is certain, that no lately published work has done so much to advance the progress of this branch of surgery as this one of Professor Donders, so ably translated for us by Dr. W. D. Moore.

[a] *Vide* Revue de l'Ophthalmolgie Contemporaine Par le Docteur Anagnostakis. Annales d'Oculistique, Tome liii. Juin, 1865. P. 270.

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.[a]

DR. CHURCHILL, President.

Malignant Osteosarcoma of the Tibia.—DR. HAMILTON exhibited a specimen of malignant tumour engaging the upper part of the tibia. The subject of the disease was a young woman, twenty-three years of age. Three years ago she became affected with a pain in the leg, below the knee, particularly when walking, and she came into hospital under the care of his colleague, Dr. Hutton. There was then a painful tumour in the upper part of the limb, but it was inconsiderable in size; it was blistered, and she got a good deal better, and left the hospital. A year after she first felt the pain, she was on a ladder, and her foot slipped, and came down flat on the ground. She experienced a good deal of pain in the seat of the disease, and from that moment the tumour rapidly increased in size. When the tumour was small the patient could walk about, but after the fall from the ladder the limb became perfectly powerless. When she came into the hospital the second time there was a very large tumour, which at first sight appeared as if it were an enlargement of the knee-joint. A little examination, however, proved that that joint was not affected. The movements of the articulation were but little interfered with, and the patella moved freely over the condyles of the femur. The disease occupied nearly the upper half of the leg, and formed a very large lobulated tumour. Some of the lobules were the size of a walnut, others as large as half an orange; they were also discoloured, presenting a livid, red appearance. The tumour felt soft and elastic; and this elasticity resembled so much the presence of fluid that one would be inclined to say, "There is a collection of fluid here that ought to be let out." However, this elastic, pseudo-fluctuating feel often arose in malignant structures in which there were no fluid contents The patient suffered no pain; the tumour was tender to a

[a] These reports are furnished by Dr. R. W. Smith, Secretary to the Society.

very slight degree, and without pulsation. A careful examination failed to detect the presence of glandular or visceral disease. The young woman, though not robust, looked healthy; the circulation was quiet, and there was reason to hope (if hope of a favourable issue could be entertained in a case of malignant disease) that amputation might be the means of saving life. It was evident, from the size and aspect of the tumour, that the integument would soon give way, when the chances of success from operation would be greatly diminished. The limb was, therefore, removed by the circular incision, in the lower third of the thigh. There was no disease in the femur where it was sawn across, but the quantity of medullary matter was very considerable, and the compact tissue remarkably thin. When the knee-joint was laid open the investing cartilage of the femur was found healthy in all respects; and the same might be said regarding that of the patella, with the exception of a small spot in its centre, where absorption had begun. The bone had been pushed upwards by the growth of the tumour. A section carried through the centre of the tumour, and through the shaft of the tibia, disclosed a well-marked encephaloid structure, together with a number of cysts. The latter occupied the most inferior part of the morbid growth; they were of various sizes, the dimensions of some being considerable: their walls were partly bony and partly membranous; they contained, for the most part, a dark fluid blood. The encephaloid portion of the tumour was of a brownish-yellow or tawny hue, and scattered throughout it were circumscribed masses of coagulated blood.—*January* 7, 1865.

Leucŏcythemia, with Cirrhosis of Liver.—Dr. Hayden said, that the specimen before the Society was taken from the body of a gentleman who had been a veterinary surgeon in the army, and served in India for seven years, and who came under his care on the 18th of September, 1862. He returned from India in the early part of that year, invalided, and had undergone a course of mercury for secondary syphilis, in London, after his return. He was of exceedingly delicate frame, and was pale, nervous, and considerably emaciated. His appetite was capricious; his bowels were irregular, and his pulse was feeble. He only procured sleep by the use of opium. An enlargement of the spleen was the only organic disease Dr. Hayden was able to detect. He was under his treatment for nine months. He suffered during this period from neuralgic pains of a slight character in several parts of the body; from periostitis of the tibia and of the cranium; and from an attack of testitis. In June, 1863, he went to the sea-side, and remained there six months. During this time his condition was similar to that already detailed. The urine was normal, with the single exception that it contained a large quantity of lithate of ammonia. He passed the winter of 1863 in private lodgings in Dublin, and went to the sea-side again in the following

season, where he remained six months. He was attacked with cachectic ulcers in both legs, of a formidable character, which, probably, were the result of the former syphilis, and about this time œdema of the feet appeared. He was frequently unable to take any food whatever. In the summer of that year he took the opinion of an eminent practitioner in Dublin, who advised him to go to Baréges and drink the waters there. He did so, and returned at the end of six weeks greatly debilitated. His face and skin were exceedingly pallid. The ulcers had healed during his absence. His extreme emaciation was due mainly to the fact that while in Baréges he suffered from continual diarrhea, due to the use of the waters. In September, 1864, he (Dr. Hayden) saw him again, when he found, in addition to anasarca of the lower extremities, which had been previously noticed, that there was effusion into the peritoneum. He became a private patient in the Mater Misericordiæ Hospital on the 23rd of last November. Whilst in hospital he suffered from a most excruciating pain in the abdomen, caused by flatulent distention of the large intestine. About this time Dr. Hayden examined the patient's blood, by taking a drop from the point of his finger, and he found, on examining it under the microscope, that the white corpuscles were greatly in excess. The red corpuscles were almost outnumbered by the white. This fact, taken in connexion with enlargement of the spleen, induced him to think that he had to deal with a case of leucŏcythemia. No suggestion was made by his colleagues, who saw the patient in consultation with him, which induced him to depart from that view of the case. The accumulation of fluid in the peritoneum increased rapidly; his debility became extreme; the bowels were generally constipated, and the dejecta of a white colour, and exceedingly fetid. There was no trace of jaundice. He died on the 21st of last December. On making a *post mortem* examination, the left pleural cavity was found distended with serum, and the lower part of the corresponding lung was compressed. The right lung was emphysematous. Of this there was no evidence during the progress of the case, and it was obviously an occurrence that took place at a very late period of his life. The right lung presented an anatomical feature of interest—namely, that it consisted of five lobes instead of three. The heart was small, scarcely larger than that of a child five years old. The right cavities were normal, but the left ventricle was greatly contracted in diameter, and its walls thickened. The abdomen was found to contain an immense quantity of fluid. The spleen was found greatly enlarged, as had been anticipated; but on examining the liver he discovered a condition for which he was not prepared, namely, that the organ was in an early stage of cirrhosis. This condition was quite distinct on the inferior surface of the right lobe; the upper surface did not show it so much, still it could be traced, and along the edge it was manifest. The gall bladder was distended with bile. The posterior

surface of the bladder was found to be connected with the anterior surface of the rectum by a band of fibrin, and attached to the posterior false ligaments of the bladder he found four bodies of a very remarkable character. They were yellowish in colour, smooth on the surface, and varied in size from a hazel nut to a pea. Each was attached by a very fine pedicle to the peritoneum. When removed from the body blood issued from the interior of these bodies. On examining them under the microscope, he found that the surface consisted of serous epithelium, and that the yellow, friable substance forming the cortex was composed of cells varying in diameter from $\frac{1}{1332}$ of an inch to $\frac{1}{1777}$ of an inch. They were distended with fatty molecules. There was no fibrinous structure whatever in these bodies. The medullary or central portion consisted of a plexus of fine capillary blood-vessels. He could not find anything described as resembling them, with the exception that Rokitanski describes some bodies of this kind as of a strumous character. The structure of these bodies would lead to the conclusion that they were of that nature. It was possible that there was first congestion or vascular distention of particular parts of the peritoneum, that a deposit was formed round that as a centre, and that the epithelium was raised in that way, and the plexus of fine blood-vessels thus became encapsuled. He offered this merely as a hypothesis, in the absence of any more satisfactory explanation. The kidneys were in an early stage of fatty degeneration, and one spot was to be seen by the naked eye consisting of fat globules. They were of considerable size, and the right one was taken from the body without its supra-renal capsule, which was found attached to the inferior surface of the right lobe of the liver.

This might explain the occasionally reported absence of one of the supra-renal capsules, which, in these instances, probably remained attached to the liver, and escaped notice. This case was of interest in many respects. In the first place it was a case of cirrhosis of the liver in a man reported to be of exceedingly temperate and regular habits. Secondly, the fact that this cirrhosis of the liver was not detected or satisfactorily diagnosed, owing to the absence of jaundice or bile in the urine, and of hemorrhage from the stomach or bowels at any period. He was led to suspect that there might be disease of the liver, from two circumstances—one, the distention of the superficial veins of the abdomen and chest, and the other that the stools exhibited a want of bile, and the dejecta were exceedingly fetid.—*January 7th*, 1865.

Cerebral Agenesia.—Dr. Peebles said, the case which he had the honour to bring under the notice of the society was one of cerebral agenesia. It had excited some interest at the other side of the Channel, where he believed a true diagnosis of it had not been made. He was happy to say that a distinguished member of the society—Professor

Law—was the first who stated its real nature. The subject was a girl aged nine years and ten months. Up to the age of eleven months, she was considered to be remarkably healthy and well-formed, and presented no appearance of paralysis. About this time a general practitioner prescribed low diet, with small doses of grey powder, for a feverish attack accompanying dentition. Soon after, for a slight cold, he directed tartar-emetic, and also two leeches to be placed on the dorsum of the foot. The low diet to be continued, and the leech-bites to be sponged with warm water to encourage bleeding. The attendants found it impossible to arrest the hemorrhage, and convulsions, followed by coma, were the result. Further advice was obtained, and then the head and spine were blistered. At this time the parents of the child, who had been from home, returned, and succeeded in getting some beef-tea, &c., into the stomach. Re-animation slowly returned, but it was found that all power of voluntary motion was absent. In the course of a year the left side recovered, but the right continued paralyzed. There was some difference in the thickness of the limbs, but none in the length. She now began to walk, dragging the right leg; and from want of power in the side met with several severe falls, the head generally coming to the ground with violence. On one occasion she fell out of bed head foremost; this was followed by epilepsy, which continued up to her death. At first the fits came on every ten minutes during the twenty-four hours. Afterwards they decreased in number, but increased in violence. The approach of a fit could be predicted by the development of mischievous propensities—a tendency to torment animals, or to strike people when they least expected it, and by considerable cunning. There was no scream before the fit. She always gave notice when it was coming on. The tongue was never bitten. There was a discharge of fetid perspiration, and seldom any dulness or drowsiness when the fit subsided. Her mental capacity was of the average; in some matters she was very intelligent, and had made some progress in her education.

She had been seen by various medical men, who attributed her ailment to either the loss of blood causing the development of tubercle in the brain, or to atrophy of the brain from the same cause. At the age of six years the left side again became paralyzed, as well as the right; but under tonic treatment the power over it returned. In last September Professor Law happened to be in the neighbourhood of where she was, and expressed a desire to see her. He was at once struck with a slight want of symmetry in the sides of the head, although measurement showed no difference, and pronounced it to be a case of congenital malformation of the left lobe of the cerebrum, similar to those which Professor Smith had brought before the society on several occasions. He (Dr. Peebles) first saw the child about two months ago, when she came under his care. Up to that time she had been able to walk three or four miles in the day

with the assistance of a servant to hold her hand. There was no difference in the thickness of the limbs, as she had grown tall and thin, but the heel of the right side was raised from the ground by the contraction of the ham-string muscles. The right arm was kept in the flexed position, with the hand hanging down; but by a strong effort of the will she was able to use it for various purposes. This, however, did not last long, the strength seemed to leave it very soon. There was a peculiar bulging of right side of the forehead, and the right cheek was drooped.

Although there was considerable power of voluntary motion in the right side, the galvanic stimulus did not appear to have any effect on the muscles, and wasted as they were, their contractive power in a fit was very great.

During the damp weather in November symptoms of inflammation of the membranes came on, quickly followed by those of effusion; but no pain was ever complained of, except a deep-seated one between the eyes.

She lingered for six weeks. Diarrhea set in, and bed-sores appeared wherever there was the least pressure, even over the left malar bone, where the cheek rested on the pillow.

As Professor Smith is our chief authority on this affection, and as the case differed in some respects from those which he has published, I requested him to make a *post mortem* examination. He (Dr. Smith) says:—"The right side of the forehead and of the head generally seemed more prominent and bulging than the left. On removing the calvarium, and making an opening through the dura mater, a large quantity of serum flowed out; and on removing the dura mater a layer of recently-formed coagulable lymph was seen covering nearly the entire of the left hemisphere of the brain. A similar material, mixed with serum, filled the sulci between the convolutions of the brain on the left side. The left hemisphere was much smaller than the right, more especially in front, the anterior lobe of the right side projecting at least half an inch in front of that of the left side. On making a vertical section of the left hemisphere a cavity or cyst was found in its substance, capable of holding a filbert. It was filled with serum, and lined by a dark brown membrane. The membranes of the brain adhered closely to that portion of the surface of the hemisphere that corresponded to the cyst. The latter was evidently the consequence of an original arrest of development, as was also the shortness of the left anterior lobe, and the generally small dimensions of the convolutions. The roof of the left orbit was much more prominent, internally, than that of the right, and the crista galli was strangely deformed. It was enlarged and curved in such a manner as to be placed to the left of the middle line, and nearly obliterate altogether the fissure for the passage of the nasal nerve."

This case differs from those which have been published by Professor Smith and others, in the absence of rigidity, and in the degree of

voluntary motion in the affected side—in the fact of the cyst or deficiency being in the substance of the brain, and not connected with the ventricle; and in the amount of mental development which was equal to the average of children of the same age. The peculiar appearance of the crista galli closely resembles the delineation in the plates published by Professor Schroeder Van der Kolk of his case of atrophy of the left hemisphere of the brain. The roof of the left orbit was also altered in the same way, but the resemblance goes no further. In his case the calvarium was thickened; in this case it was, if anything, thinner than natural. Measurement showed no difference in the sides of the skull; the space where the brain was deficient was filled with serum.

There was no appearance in the brain to account for paralysis on the left side. In the first attack the loss of power on that side was evidently the result of great prostration, as it was, most probably at the second attack, for at that time she was exhausted by diarrhea and hectic fever.—*January 14th*, 1865.

Fibrinous Tumour.—Mr. Collis said the specimen which he had to exhibit was removed from the breast of a married female, aged twenty-seven. It was an example of what was described, many years ago, by Mr. Hey, as fungus hematodes. In Mr. Hey's description of it, however, two distinct classes of tumour were mixed up, and not sufficiently differentiated. There was the genuine encephaloid cancer in a state of fungation—a condition to which every tumour was liable; and there was, secondly, the fibrinous, or hemorrhagic tumour—a tumour different from encephaloid cancer, and extremely rare in the breast, although met with frequently in other situations. There was another peculiarity about this specimen in which it differed from the ordinary form of the disease met with elsewhere. Generally speaking, this variety of tumour was encysted—the hemorrhage takes place into the cyst—the clots become organized, and cell-growths take place in them; whereas, in this instance, so far as could be ascertained, there appeared to be no proper cyst, the tumour being diffused through the breast. The history of this case was briefly as follows:—On September 28th, 1864, he first saw this patient at the Coombe Hospital, where she was under the care of Dr. Kidd. She had then a large tumour of the right breast, which arose from a blow received in January, 1864. This caused a small kernel, which increased with tolerable regularity to the size of a small child's head. The tumour was globular in outline, and was much discoloured on the surface with numerous small varicosities, such as was to be seen in integument when distended over a tumour of any kind. It was remarkably heavy, and this was a special characteristic which distinguished it from genuine cancer. It could be moved about and lifted up from the pectoral muscle. Over much of the surface there was a

distinct fluctuation, but the deeper portions of the tumour had a very firm, doughy feel. There were no enlarged glands, nor any pain experienced except at the menstrual periods, in August and September, when the tumour grew with increased rapidity. The diagnosis which he made was that it belonged to the class of fibrinous tumours. The case of fungus hematodes of the tibia, lately exhibited by Mr. Hamilton, was the same disease as this; but in that instance the morbid growth was developed within cysts. The patient left the Coombe Hospital, and in December was admitted into the Meath Hospital, under the care of Mr. Collis. The centre of the tumour was now occupied by a large slough, portions of which came away from day to day. She complained, as before, of the great weight of the tumour, and of the unpleasant smell that proceeded from it. The sloughing continued, but without any diminution in the size of the tumour; it grew as rapidly as portions of it perished. There was no pain experienced except that attending the sloughing. There were no enlarged glands until the day before the operation, when the glands in the axilla became enlarged and inflamed, as they naturally would from the contiguity of so large a tumour in a state of high inflammation. He removed the tumour on the 20th Dec., by a circular incision, which in these large tumours Mr. Collis thought gave the best means of arranging the cicatrix in the most suitable direction. The wound rapidly healed, and was now all but well, the cicatrix being almost linear. He had already stated that this tumour belonged to one of two classes described by Hey as fungus hematodes. It was a remarkable fact that this eminent surgeon was able, even at that period, to distinguish the tumour in many instances as a non-cancerous one. His close observation of nature enabled him to make this distinction between it and the ordinary fungating cancer; but later writers had not been so careful, and hence the name of fungus hematodes has been applied indiscriminately to encephaloid cancer, and to that even when it was not fungating. This tumour was extremely heavy, much more so than an encephaloid tumour of the same bulk would be. Now, if they examined its component cells under the microscope, they would find that they were small, with sharp outlines, and a good example of a tolerably dense and firm lymph cell. They were not easily broken up by chemicals or by pressure, nor did they decompose and perish as rapidly as the cells of cancer. On examining the cancer cell under the microscope they found that it was a large, soft, weak, and delicate cell, and probably light—in other words, that there was the same amount of material in the larger cancer cell as in the smaller lymph cells; hence it was very intelligible that encephaloid cancers should be light as compared with these fibrinous tumours. Mr. Collis had termed this tumour, from its structure and its tendency to bleed, the hemorrhagic fibrinous tumour, and by these names it is now recognized by some other surgeons who have met with it.—*Jan.* 14, 1865.

Tuberculosis.—Dr. Haughton brought under the notice of the society specimens of extensive strumous disease removed from the body of a large baboon (*Macacus Nemistrinus*), who had lately died in the Zoological Gardens. It was a common opinion, and published in many books, that tubercular disease was the usual cause of death among monkeys in this country. He had found that tubercle was of very rare occurrence amongst those that died during the last six years in the gardens of this city. The present was the first case he had met with in which the cause of death was tuberculosis. The animals whose bodies he had examined had generally died of one or other of two causes—of scurvy, arising from the sameness of the diet to which they were subjected in confinement, or from pneumonia, caused by a sudden access of cold in the damp weather of this climate. The macacus before them took ill about four weeks ago, and became evidently worse from day to day, though various remedies were tried with him, and he died rather suddenly on Friday morning. On examining the body he found that most of the thoracic and abdominal viscera were the seat of tubercular disease; miliary tubercles occupied both lungs throughout their entire extent. The progress of the disease had been such as to show that it appeared latest in the lungs. It was incipient in the lungs, and two or three deposits of a similar nature existed on the surface of the heart. The liver consisted of five lobes, four of which were filled throughout their substance with hard tubercular masses, and tubercles were scattered throughout the substance of the spleen. The mesenteric glands were conglomerated into one large, hard, tubercular mass; yet there had been no diarrhea during the progress of the disease, and exceedingly little cough. Dr. Haughton said that this was the first case that came under his observation, after a large experience in the dissection of these animals, of death arising from tubercular disease. The knee-joints exhibited the anatomical characters of chronic rheumatic arthritis. The patella was displaced outwards, and the outer margin of the external condyle of the femur absorbed on each side. It was remarkable that on the dissection of the lion, which died of fever a year ago, they also found rheumatic arthritis in the shoulder-joint, and the condition of the diseased bone was identical in the two cases. It was almost certain that this disease originated in a state of confinement, for the bone was completely eroded, and showed none of that polished ivory surface which it would be likely to have if the animal were capable of exercising and in a state of nature.—*January* 14, 1865.

Disease of the Aorta.—Dr. Head brought under the notice of the society a case of atheromatous and calcareous deposits in the aorta. The patient in this case was admitted into hospital in an almost dying state; his general appearance was that exhibited in an advanced stage of Bright's disease; he had a pallid countenance, feeble pulse, slow breathing, and

albuminous urine, with a slight degree of anasarca. On examining the urine, however, it was found to be normal in quantity and density; the albumen was small in amount, and there were no tube casts. The action of the heart was very feeble. There was dulness on percussion over a much larger surface than usual, and the impulse was very diffused, but there was no abnormal sound in the heart or aorta. This might be due to its feeble action. Upon examination, *post mortem*, it was found that the valves of the aorta were perfectly healthy; the atheromatous and bony deposit did not extend beyond the sulci at the back of the semilunar valves. The valves themselves were sound, and the aortic opening was not dilated, although the aorta itself was very much so. There was some degree of dilatation of the right ventricle, and very great hypertrophy of the walls of the left, which were more than three-quarters of an inch thick. Probably, if there had been a longer time to observe this case, and the action of the heart had been stronger, the usual sounds would have been recognized. There was extreme atrophy of the spleen; it was smaller than any spleen described, with the exception of one recorded by Andral. The structure was more dense than usual, and there could be traced fibrous bands through it, somewhat the same as in cases of cirrhosis of the liver. On the surface lymph was diffused, and there were small deposits of a cartilaginous hardness. The kidneys were hard and lobulated, and the left contained several cysts.—*Jan.* 14, 1865.

Apoplexy.—Mr. Adams detailed the following case, and exhibited the morbid specimens:—

A coachman, aged sixty-three, was admitted into the Richmond Hospital, December 9th, 1864, under the care of Mr. Adams, complaining of great sickness of stomach and dizziness in his head, also of severe pain in the left eye; he had continual thirst and constipated bowels. He was of full habit, but has lost flesh very much lately, and has now a pallid appearance. About three months ago he began to lose his appetite and suffer from sickness of stomach; his eyes at this time, as he expressed it, became slightly clouded, and the left one, a few days ago, became very painful. He then applied to Mr. Adams who admitted him into the Richmond Hospital. He stated that some years ago he had suffered from pains in the loins, but these had been relieved by the use of flaxseed tea and emollient poultices. Up to the time of his admission he had been able to retain his place and attend to his horses. On admission there was no pain nor tenderness anywhere in the abdomen; no anasarca; the left eye was inflamed, and there was much subconjunctival serous effusion; the pupil was dilated, and the eye amaurotic; the tongue was foul; pulse 92, and intermittent; the urine was high coloured; density, 1013; it deposited on cooling a dense, flocculent, red material; it contained albumen in considerable quantity, and when microscopically

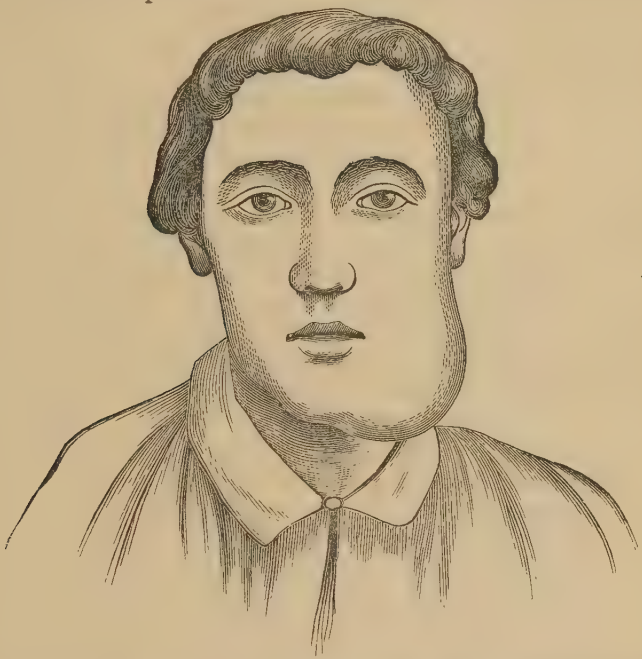
examined blood corpuscles and epithelial casts of the tubuli uriniferi were discovered. Under treatment the blood disappeared from the urine, and the eye became much less inflamed, and the effusion in a great measure disappeared, but the amaurotic condition remained and the albumen still was present in the urine. On the 8th of January he appeared better, but exceedingly weak; his pulse was 94 and intermittent, and he had lost flesh considerably since his admission. On the morning of January 11th, about four o'clock, he was found dead. One of the patients near him said that he observed him to snore louder than usual about three o'clock that morning. An examination of the body was made about twenty hours after death. The heart was large, and weighed 22 oz.; the surface was fatty, and there were some atheromatous deposits in the aorta; the muscular tissue of the heart gave way under the slightest pressure; all the valves were perfect; the liver was of its normal size, but the kidneys were enlarged, and weighed 7 oz. each. On section the surface did not present the usual difference in appearance between the cortical and tubular portions, the pyramids were interspersed with fat; the external surface was of a pale colour mottled with yellow, and both organs were enveloped by large masses of fat. A large quantity of effused blood was found at the base of the brain, and also a firm clot was seen occupying the position of the "circle of Willis." The arteries of the brain contained dark-coloured blood; their coats were somewhat thickened, and were of a yellow colour in various parts. A large clot of blood was found in the cerebellum, very near that part where the pons varolii and cerebellum join superiorly and laterally; here the upper portion of the crura and neighbouring part of the cerebellum had undergone some laceration of structure, into which parts an infiltration of blood had occurred. The source of the bleeding could not be traced to any particular vessel.—*January* 14, 1865.

Cystic Disease of the Lower Jaw.—Dr. Fleming said that the drawings which he begged to lay before the society, and the accompanying morbid specimens, illustrated the appearances present in a case of "Cystic Disease of the Lower Jaw," previous and subsequent to its removal. The subject of the disease was a young man, aged eighteen years, who had always enjoyed excellent health, and complained only of the deformity produced by a tumour on the left side of his jaw. On enquiry into the history of this tumour, it appeared that about eight months previous to his application at hospital he discovered a small immovable growth, about the size of a pea, on the external surface of the jaw, somewhat about the line of the second molar tooth. It was unaccompanied by pain or uneasiness, and, gradually increasing in size, at length formed a tumour which extended from the chin to beyond the angle of the jaw; the integuments were in every respect normal, and perfectly movable over the morbid

growth, which was obviously identified with the bone. On examining the inner and outer aspects of the jaw, the body of the latter was found to be considerably and widely expanded, especially towards the mouth and the side of the tongue, but the boy did not experience the slightest difficulty in mastication or deglutition, or in any of the ordinary movements of the tongue. It was, moreover, remarkable that the alveolar portion of the jaw was not materially implicated in the disease, and that the teeth contained in it, with the exception of the last molar, which was a little loose, were firmly fixed in their sockets. The crackling feel on pressing on different portions of the tumour, noticed by Dupuytren as characteristic of the osseous cysts of the jaw, was remarkably distinct in this case. The consideration as to the most advisable operative procedure was of some moment, namely, whether the tumour or cyst should be treated according to Dupuytren's suggestion, and a cure be thus attempted, or whether, on the other hand, the portion of the jaw involved in the disease should be wholly removed. On close investigation, however, it was quite evident that the body and base of the jaw were engaged in the tumour, their natural outline being wholly effaced, hence, that any attempt to effect a cure by laying open the cyst, or removing a portion of it, would have ultimately, in all probability, failed. The portion of the bone intervening between the symphysis and the lower half of the ascending ramus was, therefore, removed, and the society had now an opportunity of observing the pathological characters of the tumour. The altered shape of the angle and body of the jaw—the peculiar conformation of the cystic degeneration present—the varying consistence of the walls of the cyst, in some portions a mere osseous shell, and in others devoid of a particle of bony material, were well shown in a section of the tumour made by Professor Smith. The cyst was *unilocular*—contained a brownish coloured fluid, and was lined with a delicate and vascular membrane. With the view of contrasting the *multilocular* form of the disease, Dr. Fleming exhibited an admirable drawing of the latter in a case which was brought under the notice of the society by Dr. Hutton, during the last or previous session, and in which the removal of a similar portion of the lower jaw of a young woman was followed by complete success. Dr. Fleming remarked that it was desirable to take every advantage of studying the pathology of this disease, and thus to be enabled to ascertain its presence at the earliest period of its growth; hence, he thought that specimens illustrative of it should be brought under the notice of the society when ever an opportunity was afforded of so doing. Cases such as those operated upon by the late Mr. Cusack were now rarely, if ever, to be met with, and this was in a great measure attributable to the improved state of our knowledge respecting the pathology of the disease, its detection in its earlier stages, and its appropriate treatment.—*January* 21, 1865.

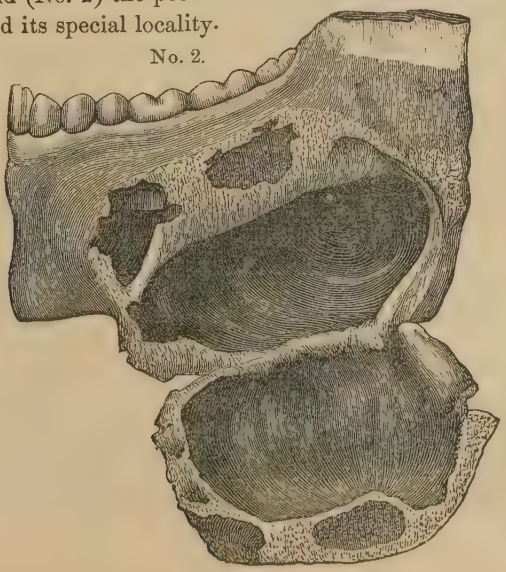
The accompanying woodcuts of Mr. Oldham are faithful delineations from Mr. Conolly's drawings. The first (No. 1) exhibits the deformity present before the operation.

No. 1.



The second (No. 2) the peculiarities of the section of the tumour when removed, and its special locality.

No. 2.



Melanoma of the Eye-ball.—Mr. Wilson said the specimen of disease of the globe of the eye which he now laid before the society had been removed from a female, aged sixty, a patient in St. Mark's Ophthalmic Hospital. The history of her case was as follows:—About eight years ago she was seized with very violent pain in the right eye-ball, which for several days continued unabated; the lids swelled to a considerable size, but there was no purulent discharge. After a fortnight, during which she was treated by leeches and purgatives, the swelling subsided and she got perfectly well, vision not having been affected. After the lapse of three months she got a similar attack, and during eight years subsequently she got repeated and violent attacks of the same description, after each of which, for four years from the commencement of the pain, the power of vision became diminished, and then was totally lost. Sometimes the pain was so intense, and the general constitutional disturbance so great, and of such long duration, that she was confined to her bed for months. The eye then ceased to be the source of much uneasiness until June, 1864, when she experienced violent pain in it, which extended over the side of the head, and was somewhat of a neuralgic character. There was no protrusion of the globe of the eye at this time. In July last she noticed, at the anterior inferior part of the orbit, and beneath the line of the lower lid, a small tumour, which increased in size until a month ago. When she came to the hospital the condition of the eye was as follows:—The ball itself was slightly protruded and nearly motionless, turned upwards and inwards, and completely concealed by the upper eyelid, which drooped over it, and the veins in which were enlarged and the skin of a dusky hue. The eyeball was discoloured, but the cornea was clear; the iris was of a dusky slate colour, the lens yellow and opaque, and projected upwards against the back of the cornea. The conjunctival vessels on the globe were large and tortuous; bulging out over and depressing the lower lid was a large red tumour, composed of conjunctiva. When the finger was pressed on the lower eyelid a large, firm, round mass could be felt and grasped by the finger and thumb, and when so grasped and moved the eye participated in the motion. The woman had the peculiar yellowish skin and look so often characteristic of malignant disease. There was no gland enlarged, and the diagnosis was, that the case was one of melanosis (in consequence of, or altogether with, choroiditis), commencing in the choroid, bursting through the sclerotic, and finally encroaching on the orbit and pushing the eye-ball upwards and inwards. The entire mass was removed by Sir William Wilde last week, and the case had gone on very satisfactorily. The specimen presented the characters of melanotic disease, the interior of the globe was filled up completely with a soft black mass, the retina and choroid being perfectly broken down and unrecognizable; the lens was small, opaque, and close up against the iris, which structure lay in apposition with the

back of the cornea. At the inferior part of the globe the sclerotic presented a funnel-shaped opening, through which the mass in the vitreous chamber was continuous with a firm lobulated tumour, situated external to the globe, and extending chiefly downwards and forwards. The optic nerve, which was partially surrounded by this tumour, was quite black and disorganized; the external tumour was firm and grey with black deposits scattered irregularly through it. Spindle-shaped nucleated cells, large black globular cells, and granules were largely developed in the tumour.—*January* 21, 1865.

Aneurism of the Aorta.—Dr. Hayden brought under the notice of the society an example of aneurism of the arch of the aorta, taken from a female, aged thirty-one, who was admitted into the Mater Misericordiæ Hospital on the 2nd of January, and died on the night of the 15th of that month. All he could learn of her history was the following:—About nine years previously she had an attack of hoarseness and loss of voice, from which she quickly recovered, and enjoyed comparatively good health up to the 21st of last December. On the night of that day she was exposed to cold, and on the following morning found she had again lost her voice; she had also cough, and for this she was treated in one of the city dispensaries with very little beneficial result. The hoarseness and cough continuing, and her strength failing, she sought admission into hospital. Her appearance indicated great distress, and she was haggard in the extreme; her face was pallid, with a tint of lividity; there was complete aphonia, with loud stridulous breathing, audible at a considerable distance from where the patient lay. On the slightest movement of her body, or on exposure of the chest for the purpose of examination, a fit of spasmodic cough ensued, accompanied by copious mucous expectoration of a frothy character. The right radial pulse was 108, weak, but regular; the left was entirely suppressed, as likewise the left brachial pulse, as he thought, on that occasion; there was no dysphagia. She was unable to lie down, and on attempting to do so a fit of difficult breathing came on. With the symptoms before him, and the history he had received of the case, he had in his mind a suspicion of intra-thoracic tumour. He made a careful examination of the chest, but detected no signs to confirm this suspicion, except that respiration in front and under the left clavicle was more feeble than on the opposite side, and also that a shade of dulness on percussion was perceptible on both sides. The bronchial stridor was so intense that it was next to impossible to make a stethoscopic examination of the chest. The case went on from day to day without any improvement. He found that, on the 3rd of January, the day after her admission, he had made a note that she swallowed in his presence some beef-tea with bread soaked in it, a fact to which he wished to direct special attention. On the 7th of

January, on entering the ward he noticed a marked contraction of the left pupil, which continued even in the shade. He made a careful examination of the larynx with the laryngoscope; it was found perfectly healthy, and both vocal cords moved freely in respiration. At this time he asked his colleague, Dr. Hughes, to see the patient with him. Dr. Hughes made a most careful examination of the patient's chest, and declared his opinion that, with the exception of the physiological symptoms of contracted pupil and suppressed pulse, there were no physical evidences of intra-thoracic tumour. This was Dr. Hayden's opinion, notwithstanding that he found dulness on percussion at both sides and above, and feeble respiration on the left side. On that occasion Dr. Hughes' attention being directed to his (Dr. H.'s) suspicion, ascertained that there was very feeble pulsation in the left brachial artery at the bend of the elbow. This was an important fact as distinguishing between the two causes for suppression of pulse, namely, an abnormal distribution of the arteries and arterial obstruction. It settled the point affirmatively as regards the latter, showing that the suppression of the pulse was not due to an unusual distribution of the arteries. On the following day he found there was partial ptosis on the same side; the patient's eyelid drooped so that the equator oculi was reached by the lower margin of the upper eyelid. At this time it was noticed that during the fits of spasmodic dyspnea and cough, which were caused by any movement of the patient's body or an exposure of her chest, pus was discharged in large quantities; it gushed from the mouth on these occasions, followed by momentary relief. The pulse became weaker, the respiration more rapid; and, notwithstanding that the patient took food in moderate quantities, she rapidly sank, and expired on the night of the 15th, dying in a paroxysm of dyspnea.

The examination of the body afforded a satisfactory explanation of all the symptoms, as well negative as positive, that had marked the case. On opening the chest it was found that both lungs were emphysematous. The left was adherent to the parietes of the thorax by recently effused lymph, and its base was in a state of pneumonic consolidation, and from the surface of a section of it pressure forced out purulent matter in considerable quantity. The larynx was perfectly healthy. There was a deposit of fat on the surface of the heart; the valves were healthy; the left ventricle contained a quantity of decolourized fibrin. A large aneurismal tumour sprung from the transverse portion of the arch of the aorta, to the left of the arteria innominata, and extending to the origin of the subclavian, which was pressed upon for one inch from its origin; the tumour sprang from the upper portion of the arch, involving the superior and posterior walls of the vessel; it extended back to the vertebral column, and turned from the left side to the right, passing through the posterior mediastinum behind the trachea, which escaped pressure, and carrying with it the œsophagus,

which was stretched over the tumour and carried with it into the right pleural cavity. The appearance presented by the parts was exceedingly well represented in the drawing by Mr. Conolly which he now exhibited. The aneurism, which had received the impression of the spinal column, was of the size and figure of a large peach. The left carotid sprang from the tumour, but escaped pressure; the left subclavian was compressed and all but obliterated as far as the origin of the left vertebral artery; and thus was afforded a satisfactory explanation of the all but complete suppression of circulation in the left upper extremity. The œsophagus, carried from left to right on the tumour, admitted the finger to be passed down its canal with great facility; from the tumour it passed downward, and to the left side, into the posterior mediastinum. The pneumogastric nerve on the right side was stretched by the tumour; the left recurrent nerve passed from the pneumogastric round the tumour, in which it became so involved that it was impossible to dissect it beyond half-an-inch over the aneurism—so that this nerve was carried from the left side across the posterior mediastinum to the right pleural cavity. It must have been greatly put on the stretch, and afforded a satisfactory explanation of the paroxysmal dyspnea and laryngeal symptoms exhibited during life.

The points of interest in the case were these:—First, the occurrence of the disease in a female, which was rather a rare circumstance; secondly, from the history of the case, the age of the woman, the smallness of the tumour, and the absence of erosion of the vertebræ, it was evidently of short duration; thirdly, that the position of the tumour afforded a satisfactory explanation of the several symptoms, negative as well as positive. They were unable to detect any conclusive signs of aneurism in front, because it had passed to the posterior wall of the thorax, and been overlapped by the emphysematous lungs anteriorly. Hence, the absence of bruit, of pulsation, and of the usual physical signs. There could be little doubt that if he had been able to institute a satisfactory examination of back of the thorax he would have discovered pulsation, and probably the evidence of aneurism, over the superior dorsal vertebræ, against which the tumour lay; but the loudness of the stridor was such that it was impossible to ascertain anything by the stethoscope. The position of the tumour explained satisfactorily the state of the pupil. It must have made pressure on the sympathetic nerve in its descent into the thorax on the left side; it must also have compressed the vertebral ganglion against the transverse process of the seventh cervical vertebra. How was the ptosis on the left side to be explained? In all the experiments of dividing the cervical sympathetic which Bernard had performed, and in those he had performed himself on rabbits, it was observed that ptosis always occurred on the same side as contraction of the pupil. He thought it probable, therefore, that both these phenomena were caused by paralysis of the sympathetic nerve.

There can be no doubt that the attacks of paroxysmal dyspnœa, which were brought on by excitement of the circulation, were due to laryngeal spasm, the consequence of increased tension of the left recurrent nerve. It will be recollected that this nerve encircled the aneurismal tumour, and was incorporated in its wall; it must, therefore, have been subjected to increased tension by the temporary expansion of the aneurism resulting from excitement of the heart's action. In the intervals between these paroxysms respiration was easy and unattended with stridor; and, as already mentioned, laryngoscopic examination showed that at these periods the vocal cords moved harmoniously and regularly on both sides in respiration.—*January* 21, 1865.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.[a]

TWENTY-SEVENTH ANNUAL SESSION.

Dr. Churchill, President.

Placenta Previa.—Dr. Telford related the following case, and exhibited the morbid specimen:—Rose Serins, a healthy-looking woman, aged twenty-six, was admitted to the Lying-in Hospital, March 5th, 1865, pregnant of her first child. The menses last appeared on the 15th of last June. At 5 a.m., on the morning of admission she was attacked by hemorrhage; the amount lost the patient cannot state. She became alarmed, and came to hospital, where she remained till 11, a.m., when, not being in labour, and the hemorrhage not having recurred, she went out. She was re-admitted at 9 p.m., of the same day, having had some slight loss during the interval. On a vaginal examination the os was found undilated. There was no uterine action; pulse 80; fetal heart audible. Ordered to remain in the horizontal position. At 2 a.m., on the 6th, a smart gush of hemorrhage took place, from which she lost from eight to ten ounces of blood. On examination the os was found slightly dilated; no presentation discernible. An attempt was now made to rupture the membrane, which was unsuccessful. On slightly dilating the os the placenta could be distinctly felt covering its surface. The nature of the case being now evident, plugging was resorted to, by the means usual in the hospital, namely, a large conical sponge. 10 a.m.—The plug was removed, a small coagulum adhering to its upper surface, but no evidence of recent hemorrhage. Os about the size of a shilling, firm, and undilatable; pulse, 84. The vagina was carefully syringed with cold water containing some of Condy's disinfectant fluid, the plug washed and

[a] These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

re-applied in the hope of checking hemorrhage, and inducing uterine action. The patient was ordered 2 ounces of wine, chicken broth, and arrowroot. At 3 p.m. the plug was removed, washed, and re-applied, no progress having been made. 7 p.m.—Ordered 4 ounces wine, and anodyne draught at bed-time. At 10 a.m. on the 7th the plug was withdrawn, and was not re-applied, no hemorrhage having taken place. Os in the same state; pulse 108, rather feeble. Ordered a saline aperient, wine 4 ounces, and chicken broth. 8 p.m.—The pulse had suddenly risen to 130; very feeble; tongue moist and furred; tenderness over uterine region; bowels moved by aperient draught. Ordered 4 ounces wine, a full anodyne, and to have a large linseed-meal poultice over the uterus. 12 p.m.—Hemorrhage to 8 ounces; plug re-applied. Ordered a second anodyne draught. At 10·30 a.m. on the 8th, plug withdrawn; no hemorrhage on its removal; os in the same state; pulse, 130, very weak; tongue moist and furred; lips dry; thirst urgent; stomach irritable; respiration tranquil, 20 in the minute; countenance anxious, and the decubitus that of one suffering from great exhaustion; fetal heart audible. 7 p.m.—Pulse 140; tongue dry and coated; thirst and irritability of stomach continues; complains of severe pain over uterus; fetal heart inaudible. Ordered 2 grains of calomel, with half a grain of opium, every four hours; brandy and a bottle Seltzer water; to have the abdomen covered with a linseed-meal poultice, wet with anodyne liniment. 9th, 10·30 a.m.—Countenance haggard; pulse 140, small and feeble; tongue dry; lips covered with sordes; vomiting of a greenish watery fluid; os a little larger than a shilling; vagina hot. To continue the pills, and to have a mixture containing prussic acid, Battley's solution of opium, and carbonate of soda; brandy and Seltzer water. 1 p.m.—Slight flooding, checked by the application of cold. 3 p.m.—Hemorrhage to the extent of 10 ounces having occurred, the plug was re-applied, the os being then about the size of a halfpenny; pulse 150, very weak; stimulants freely administered. 5 p.m.—Some blood appearing by the side of the plug, and the patient evidently sinking, it was deemed advisable to attempt delivery. The os, though not larger than a half-crown, was soft and dilatable. The pulse was imperceptible in the left wrist, but could be felt in the right, very rapid and weak. The fingers were gradually inserted through the os, and the hand was passed through the placenta, a knee laid hold of, flexion performed, and the delivery completed without much difficulty. During the operation the patient had 12 ounces of brandy. The respiration now became hurried and blowing, the pulse imperceptible. She gradually sank, and died three-quarters of an hour after delivery.

Autopsy seventeen hours after death. The body was well nourished, and did not appear very anemic. On opening the abdomen there was no evidence of inflammation found; the peritoneum was quite healthy, as

were all the thoracic and abdominal organs examined. The uterus was laid open by a ⊤ shaped incision, when the placenta was seen firmly attached to the lower part of its posterior wall, a large portion hanging down over the os, where it was torn by the hand of the operator.—11*th March*, 1865.

Practical Remarks on Puerperal Insanity. By Dr. Lalor.

Thirty-nine women, certified as labouring under puerperal insanity, have been admitted to, and came under my observation in, the Richmond District Lunatic Asylum, since I took up my residence there, as resident physician, in November, 1857; the entire number of female cases admitted in same period being 661. Nineteen cases were chronic at time of admission, being of the duration of ten years in one case, four years in one, one in two; ten months in two, nine months in one, six in one, four in two, three in two, two in four, and one in three. Twelve of the chronic cases were cases of mania, and seven of melancholia; eight have been discharged recovered, two relieved, and two died. The deaths occurred in one case from apoplexy, at the age of fifty-four years, eighteen years after the commencement of insanity, and after seven relapses; the other death resulted from phthisis, at the age of thirty-two years, and seven years after commencement of insanity.

Seven of the chronic cases still remain in the institution, as of unsound mind. Of the cases of recovery, three were cases of melancholia, out of a total of seven; and five cases of mania, out of a total of twelve. In another case, which was of three weeks' duration at time of admission, mania did not set in until three months after childbirth; this case still remains in the institution, weak-minded, and labouring under great impairment of memory. Adding this to the nineteen chronic cases, we have twenty cases, which I propose to exclude from further consideration, as I think it will be better to confine my remarks hereafter in this paper to the remaining nineteen, *which form a natural group of acute cases of a first attack of puerperal insanity, occurring within the month after childbirth, and not of more than one month's duration at the time they came under my observation.* The ages of these nineteen cases were as follow:—Twenty to twenty-five years in six cases, twenty-five to thirty in eight, thirty to thirty-five in four, and thirty-six in one case. Sixteen were married, and three single. Ten were returned as of no occupation, which in the case of nine of them, who were married, may be taken to signify that they were occupied solely with their household affairs; five were dressmakers, one a servant, one a bootbinder, one a laundress, and one a marine store dealer. Thirteen were from the city, and six from the county Dublin. Six of the cases were primiparæ; in six it was the second childbirth, in one the third, in two the fourth, in one the fifth, in two the sixth, and in one the ninth.

The form of insanity was mania in sixteen and melancholia in three cases. The duration of the disease at time of admission was—twenty-one days in two cases, eighteen in one, fourteen in two, twelve in two, ten in one, nine in one, eight in three, seven in four, five in one, and two in two. Fourteen were discharged recovered, and are believed never to have relapsed; one was discharged relieved, re-admitted, escaped whilst convalescent after her re-admission, and was kept at home by her friends as recovered. Thus fifteen may be considered to have recovered. Four died. The three cases of melancholia all recovered.

The duration of the disease in those recovered, reckoned from the date of commencement of attack to the date when they were discharged, was—in one case, 316 days; in one, 260; in one, 238; in one, 231; in one, 203; in one, 104; in one, 68; in one, 65; in one, 60; in one, 57; in one, 43; in one, 42; in one, 39; in one, 38; and in one, 34.

The length of residence was—in one case, 302 days; in one, 258; in one, 231; in one, 221; in one, 189; in one, 96; in one, 60; in one, 53; in one, 47; in one, 45; in one, 35; in one, 33; in one, 31; in one, 27; and in one, 26.

On admission, or soon after, I noted the state of the pulse in sixteen cases. In seven cases it was 120, and of these four died; in two it was 104; in two, 100; in two, 96; in one, 80; and in two, 72. I noted the state of the tongue in eleven cases; in five it was clean and moist—one of these died; in one, furred white; in two, dry and coated; in one, moist and slimy—this was a death; in one, dry and furred; and in one slightly furred and dry—this case was a death.

I noted the state of the skin in thirteen cases. In nine it was natural, and of these three died; in two, cool; in one, hot; and in one the hands were moist and warm; the rest of the skin natural in temperature—this case died. Jactitation sometimes occurred; and here, as in other diseases, was a very grave, but not necessarily a fatal, symptom. Wakefulness was a very general symptom. Œdema of the feet occurred in one case, and was followed by convulsions and death. Restlessness, great tendency to movement, excitement, talkativeness, and delirium of a lively character were, on the whole, the most usual indications of the state of the mind; sometimes a remarkable tendency to sing, or to throw words or sentences into rhythmical order, and repeat them with rhythmical cadence, was present. Sometimes a wish to destroy the child existed. Amatory desires were frequently manifested, and indecent acts and language practised. In some of those, and in other cases, there was a disposition to throw off portions of their clothing, so that dresses secured by the lock used in asylums and hospitals for the insane became necessary.

The following is a short summary of the general treatment:—Sedatives were frequently required to procure sleep, especially when the pulse

was quick, and muriate of morphia proved in most cases the best narcotic, in doses of from half to one grain, or more, according to circumstances; less than half-a-grain being seldom efficacious. Hyoscyamus was sometimes tried as a sedative; and, if the tongue was also coated, was given, in combination with blue pill, five grains of each at bed-time, generally with benefit. Tonics, stimulants, and nutritious food were generally called for, and especially when the pulse was quick. Depletion, local or general, and lowering treatment were avoided. When the bowels were constipated moderate aperients were administered, such as mild doses of castor oil, plain or combined with tincture of rhubarb; or aloes were prescribed, in the form of the compound decoction or of pill. When the complexion was anemic, iron proved the best tonic—the mist. ferri. aromat. being the best form. Quinine was also frequently and usefully given. Blisters to the nape, vertex, temples, or behind the ears were used—rarely, however, and only for the same indications as point to head mischief in bodily disease. In puerperal, as well as other forms of insanity, the grand rule for medical treatment appears to me to be, that we should be guided by the bodily symptoms, and that the same medical measures are indicated as if the same state of body were met with in a sane person. Refusal to take food occurred here as in other forms of insanity, and required to be met promptly, as weakness and debility were the usual accompaniments and greatest obstacles to recovery. Food was given three times, or at least twice, daily. I used the vulcanised India rubber syphon and tube, recommended by Dr. Tuke in his excellent paper in the 23rd and 24th numbers of the *Journal of Mental Science;* and I consider this the best mode of forced alimentation in cases of insanity, whether puerperal or otherwise. In puerperal mania, as in other forms of insanity, mechanical restraint should be avoided. Cutting the hair was not practised, unless when required for the purpose of blistering; and it is a measure which I think should be avoided in the treatment of insanity, unless there is some positive indication calling for it, which, in my mind, is very rarely the case. The loss of the hair is acutely felt by women in general, and not less so by females recovered from insanity, as being indicative of a disease to which so much degradation usually attaches. The stay in hospitals for the insane after convalescence from insanity is sometimes prolonged to allow the hair to grow, as a necessary condition in order to gain employment, or for other reasons; and therefore I think cutting the hair should be very seldom resorted to, and should be the exception rather than the rule. In convalescence from the acute form of puerperal insanity I believe it is quite safe to discharge the patient after a very short probation, once the restoration to reason appears complete and the bodily health good. I believe that relapses are very infrequent under these circumstances; and the anxiety of the patient and of her friends for a return to home is

generally very strong, and a refusal to gratify wishes so natural would be unadvisable when the danger of evil results from discharge is very small, and less in my mind than from the danger of detention. The caution I have given as to cutting the hair applies, then, with particular force in acute puerperal mania, owing to the chance of discharge being advisable before the hair, if cut, could be well grown.

The important question of prognosis is one of the deepest interest in acute puerperal insanity; and it may not be without use to examine this question here in connexion with the cases just considered, and with the bearing of their results on medical opinion in general, as well as that of some very high authorities on this subject. Dr. Gooch, in his admirable essay on the Disorders of the Mind in Lying-in Women, published in 1831, says:—"I remember the time when it was the prevalent belief among medical men that these diseases were never fatal." The same author also states "that Dr. Baillie, when consulted about a case, is stated to have remarked that the question was not, 'whether the woman was to get well,' but 'when she was to get well.'" The patient referred to died within a week. Dr. Gooch further relates, "that Dr. William Hunter, in his manuscript lectures, said—'Mania, in the course of the month after lying-in, is not an uncommon appearance, but of that species from which they generally recover. When out of their senses, attended with fever like paraphrenitis, they will, in all probability, die; but when without fever it is not fatal.'" Dr. Gooch adds:—"Putting together this statement of Dr. Hunter with my own experience, I extract from it the following meaning: that there are two forms of puerperal mania, the one attended by fever, or at least the most important part of it, a rapid pulse; the other accompanied by a very moderate disturbance of the circulation; that the latter cases, which are by far the most numerous, recover; that the former generally die."

In the general summary which I have given of the results in nineteen cases of acute puerperal insanity of the class defined by me, I have included all cases that came under this class, whether they were simple or complicated with other diseases, as I was anxious to place before the Obstetrical Society some notice of all the cases which came under my observation from their admission to the asylum, in order not to deprive the members of any information coming within the compass of a short paper such as this must necessarily be. I do not think, however, that it would be a scientific treatment of the question of prognosis, as it regards puerperal insanity solely, to include cases complicated with other serious diseases. The cases which recovered were not so complicated. In one of the cases which died, M. A. M., emaciation was present, as were also the other symptoms, as well as the signs, of advanced pulmonary consumption, of which disease she died. That she had consumption on admission was the opinion of Dr. Cruise, who recommended her to the asylum, and also

of Dr. Tuohill, one of the consulting physicians of the asylum, and my own. In a second case of death, that of E. M., puerperal metritis and peritonitis was evidenced, and recognized during life by Dr. Byrne, who recommended her for admission, by Dr. Banks, and myself,[a] and was proved, by *post mortem* examination, to have existed.

In a third case of death, that of F. F., there was œdema of the feet from the first, and the œdema subsequently extended up the legs and thighs; the complexion was waxy; the secretion of urine scanty; and convulsions and hemiplegia preceded the fatal result. These symptoms do not belong to uncomplicated acute puerperal mania, but are well known to accompany renal disease very frequently. In this case also a dry brown tongue and sordes on the lips set in about two days after admission, and indicated more fever and of a different type than the fever of paraphrenitis referred to by Gooch. I, therefore, concluded that renal disease existed and gave rise to uremia and serous effusion which were the causes of the unusual symptoms narrated, and of death. Thus in two out of the four cases which died serious complications existed beyond all question, and it was to these diseases and not to puerperal insanity that death was attributable. In the third case I submit that there were sufficient grounds for holding the same opinion. Abstracting those three cases from the seven in which the pulse was rapid, we have four cases of uncomplicated acute puerperal mania of the class under consideration with rapid pulse, but with no other grave febrile symptom. Of these, three recovered and one died. This is equal to a mortality of 25 per cent., and proves the great value of taking into consideration the rapidity of the pulse as an important guide in forming our prognosis.

The range of pulse from 100 to 104, noticed in four cases, may be considered something more than what Dr. Gooch calls a very moderate disturbance of the circulation, and may be taken to represent a class of cases intermediate between those with the pulse indicated by that expression and the pulse implied by the term "rapid pulse." No death occurred, and recovery followed in all those cases just as in those where the pulse did not exceed 96, or the rate which is generally understood to be indicated by the term "moderate disturbance of the circulation." The general proposition as to the absence of danger to life, and the almost absolute certainty of recovery of reason in cases of acute puerperal insanity, attended by little disturbance of the circulation, as laid down by Gooch, agrees with my own experience. Further, abstracting these cases with serious complications from the entire nineteen cases under consideration, we have remaining sixteen cases of acute uncomplicated puerperal mania; and of these fifteen recovered and one died, being at

[a] A very graphic report of this case, by my friend Dr. Banks, is published in the Dublin Hospital Gazette, for December 15, 1859.

the rate of $93\frac{12}{60}$th per cent. of recoveries, and $6\frac{4}{12}$th per cent. of deaths. I believe that at the present day there is no such prevalent belief amongst medical men as was held in Dr. Gooch's recollection (and indicated by the saying of Dr. Baillie which I have quoted), viz.—"That disorders of the mind in lying-in women are never fatal." If any one retains such a belief, the results just stated are amply sufficient to disprove it, and my paper will not be entirely useless if it should remove a very serious error of opinion from the mind of a single practitioner of medicine. At the same time, if my later amended analysis, confined to cases of uncomplicated puerperal mania, offers a more legitimate deduction from fitting facts than that presented by my first statement, of the gross results in all cases, whether simple or complicated, a more hopeful view may be taken of the disease in that form which I believe was alluded to by Drs. Hunter and Gooch, in the propositions which I have brought under the notice of the society, than was put forward by those eminent authorities in these propositions. Nevertheless, acute puerperal insanity, occurring within the month after child-birth, even when uncomplicated and brought under treatment within a month after its commencement, should not be considered otherwise than as a very serious disease; and I consider the results I have just stated in sixteen cases as, on the whole, satisfactory. That they were so satisfactory is, I think, attributable to the discriminative administration of nutritious food and stimulants, and to a like avoidance of lowering and depleting measures. This was the plan of treatment recommended by Dr. Gooch himself, and generally followed at the present day, and in the advocacy of which I do not assume to be at all peculiar, or to have any other merit than that of following the track which the general experience of the profession has pointed out as the best and safest. In writing this paper my chief object and hope have been to stimulate inquiry, especially amongst the junior members of the society, rather than to satisfy it; and it is my anxious desire to induce them to direct their attention more than is usual to the study, not alone of the very interesting form of insanity which has been the subject of this paper, but of insanity in general, of which it forms only a very small portion, as may be learned from the fact that the number of cases of puerperal insanity in Ireland, on 7th April, 1861, according to the last Irish Census, was only 37, whilst the number of cases of insanity from all causes, at same date, was 14,098.

The many great discoveries made by members of the medical profession in the anatomy and physiology of the nerves and nervous centres, have partly drawn the science of mind out of the labyrinth of metaphysics. The general dependence of mental disturbance on morbid bodily action, suggested by anatomical and physiological research, has been confirmed by medical observation of the disorders of the human body during life and after death, and hence, the treatment of insanity has become a

recognized branch of medicine. The foremost rank now held by medicine in psychology, theoretical and practical, was only acquired after a long and arduous struggle with rival claimants belonging to other professions, or to none, many of them being persons of large minds, varied acquirements, of much worldly knowledge and learning, or of great benevolence; these qualities being sometimes more or less in combination. But I believe it will be admitted that the study of psychology cannot be fully and successfully cultivated without the aid of those scientific methods which are the main sources of all medical knowledge. It follows, that as long as the healing art shall have existence so long shall the treatment of insanity be our right. It is, therefore, our duty to acquire the knowledge necessary for its correct practice; and the education of the medical student ought not to be considered complete until it has embraced the study of so-called "mental disorders."—11*th March* 1865.

Dropsy of Fallopian Tubes.—Dr. Bennett exhibited to the society a recent specimen of this. He said:—The specimen which I bring before the society this evening was taken to-day from the body of a middle-aged woman, brought for dissection into the Medical School of Trinity College. I discovered the tumour on opening the abdomen and turning aside the intestines; it occupied the cavity of the true pelvis, filling it completely, and projected upwards a short way above the brim. The tumour at first sight looked very like a distended bladder; its walls were, however, much thinner than those of the bladder. It was free from adhesion either to the intestines or walls of the pelvis, but could not be raised in the least out of the pelvic cavity, in consequence of the shortness of its pedicle and the intimate connexion of the pedicle to the uterus. In order to make out the nature of the tumour exactly it was necessary to remove it with the bladder and uterus. I now exhibit to the society the parts so removed. The tumour is a very peculiar one—its shape ovoid; the broad end, that remote from the uterus, about the size of an infant's head; the narrower end is in contact with the fundus of the uterus, and has coiled around it spirally from left to right a tube which, on examination, proves to be the Fallopian tube dilated. The tumour is evidently a simple cyst, and its cavity communicates freely with that of the dilated Fallopian tube, which is lost in the tumour, there being no part of its fimbriated extremity discoverable; the broad ligament of the uterus contains the tumour, and attaches it to the uterus. The ligament of the ovary passes directly into the cyst wall, and all trace of the ovary, external to the cyst, is wanting. There are a few adhesions about the attached part of the tumour, between adjoining parts of the broad ligament. On opening the cyst the fluid contents are found to be ordinary serum, the cavity is proved to be a simple one, and where the Fallopian tube is lost on its surface, externally, a finger can be introduced

from the cyst into it for some distance; the tube gradually contracts, and is completely impervious two inches from the uterus; in no part of the tumour can the ovary be detected, though in the part of the cyst wall which contains the ovarian ligament the remains of the vessels of the ovary can be seen. The condition of the uterus and the parts in the left broad ligament, explains, I think, the nature of this tumour. The os uteri is so contracted as barely to admit an ordinary probe; the tissue of the cervix and body of the uterus is much harder than usual, and their cavities are greatly contracted. Both Fallopian orifices are closed; the left tube, like the right, is impervious for some distance, and then dilates into a thin cyst, which retains the shape of the tube pretty accurately. The extremity of the tube is closed, its fimbriæ are gone, and it is intimately attached to the ovary by two adhesions—one, the normal one, thickened, the other abnormal. The ovary is free from any trace of disease, cystic or otherwise, and is such as we usually meet with in women past child-bearing. From such a condition of parts I think we may deduce the cause and nature of the larger tumour, even without a knowledge of the history of the case during life. The condition of the uterus and left ovarian structures shows that an attack of inflammation, probably catarrhal, caused contraction of the os uteri, obliterated the left Fallopian tube at either end, and caused it to adhere abnormally to the ovary. On the left side the progress of disease was arrested, while on the right the same attack of inflammation, having caused similar adhesions between the ovary and its duct, was succeeded by cystic growth; the extent of growth of the cyst formed thus by inflammatory adhesions, has involved the ovary with its ligament and vessels, and finally destroyed all trace of the ovary.

This form of dropsy differs in many points from those which occur in the ovary itself, or between the layers of the broad ligament, which seem to owe their origin to cystic degeneration alone, without any inflammatory process—the latter to cystic degeneration of the parovarium, the former to the same change affecting a Graafian vesicle. If my deductions from the facts of the case are correct, the previous occurrence of inflammation of the uterus and its appendages would be necessary in order to the formation of such a tumour as this before us. The occurrence of such inflammation, and of the consequent condition of the uterus, taken with the fixed character of the tumour, might assist in the diagnosis of this form of dropsy.—11*th March*, 1865.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.[a]

SESSION 1864-65.

DR. JOHN BAIN, President.

Bronzed Skin. By DR. POPHAM.

DR. Popham submitted to the society the kidneys and supra-renal capsules, with a section of the skin, taken from the back of the neck of a woman, who had died of supra-renal melasma. Ellen Donovan, aged 50, unmarried, was admitted to the Cork Union Hospital, January 22nd, 1864, with bronzed skin, which had occurred twelve months previously, after jaundice. Her olive-coloured appearance would strike any person who saw her. The back of the neck next the hair was as dark as the hue of a negress. The whole of the back was a deep brown, very marked about the hips; the anterior surface of the trunk was less tinged than the posterior. The upper part of the neck and thorax were thickly dusted with white furfuraceous scales. The colour of the arms was lighter than that of the body, the dorsum of the hands being the deepest part, and the palms of natural colour, while the nails had lost their pinkish tints. The nipples and areola were as dark as in the pregnant state; but the circle of the areolar nigrities scarcely exceeded an inch diameter, and it was distinguished from that of pregnancy by the absence of tumid follicles. The face was more sallow than brown, but the presence of inky spots on the prolabium at once fixed the attention; these patches were on the inner surface of both lips, most on the upper and along the margin of the gums, and more sparsely over the buccal mucous membrane; the tongue was pale; the conjunctiva was discoloured; one eye suffered from cataract, the other from opacity. The legs retained their natural colour; the surface of the skin felt dry and rough—no itchiness or soreness; the application of a blister always developed an increased amount of pigment.

This case continued under Dr. Popham's observation during fifteen months, and scarcely any improvement in the colour was observed in that time; the only external application of any good was lemon-juice; the internal use of this medicine also promises some advantage. The general symptoms were, however, by no means stationary. Her appetite was at first voracious; but during the last month it was abolished altogether; no vomiting occurred till towards the close, when it became uncontrollable; diarrhea, at the beginning frequent and with orange discharges, passed finally into obstinate constipation. The pulse rose slowly from 72 to 120, and the heart-stroke and sounds, became gradually weaker; thirst was almost the only symptom which suffered but little

[a] These reports are supplied by Dr. David Cremen, M.B., T.C.D., Secretary to the Society.

change; she never complained of pain. The urine was greenish-yellow, in fair quantity, clear, acid, sp. gr. 1014, average, with neither albumen nor sugar; nitric acid caused a copious purple deposit. All through she had painful depression of spirits. Phthisical symptoms set in at the close, but the most distressing complication at that period was the incessant vomiting of blackish and acid matters. Pediculi existed in enormous quantity. The mind gave way at the end, and delirium, moaning, and screaming disturbed her last moments. She died, April 30th, 1865; the discolouration having continued twenty-seven months.

The *post mortem* was obliged to be hastily conducted. Emaciation had set in with the vomiting, but still was much less than in ordinary phthisis; no change by death in the colour of the skin. On comparing the two renal capsules, the right appeared but little changed, but the left was much altered—it was pulpy and diffluent, wasted in size, and contained a cavity the size of a pea with unhealthy pus; no cheesy or gritty matter existed. The kidneys appeared normal; liver fatty; tubercles were found in the lungs; the heart flaccid and pale.

Dr. Popham has seldom seen a case in which the tedium vitæ was more marked, or the vital powers so hopelessly and helplessly prostrated as towards the close of her life.

Case of Infantile Poisoning by Tincture of Opium Treated by Extract of Belladonna. Reported by Dr. S. O'Sullivan, House Surgeon, North Infirmary.

Daniel Ryan, a child, twelve week's old, was brought to the North Infirmary on the evening of the 22nd April, 1865, at seven o'clock. At four o'clock this afternoon he got, by accident, a dose of laudanum, equal to about one scruple. Very soon after taking it, he started up, as the mother states, in a fit, threw his arms about, vomited, and then sank, as if exhausted. As the child continued in this prostrate condition, the mother took him to Dr. Allen, from whom he got an ammonia mixture, of which one or two doses were administered to him. Soon after he was brought to the infirmary. He was then in a lethargic condition; surface of the trunk of the body warm, lower extremities very much flexed and cold; eye-lids spasmodically closed, pupils contracted to the size of a pin's point. He had not passed any urine since he took the laudanum. Pulse rapid and feeble, could scarcely be felt, intermittent.

Applied cold douche to the spine and sternum alternately. This caused him to gasp for breath, and roused him. Administered strong tea and a solution of tannic acid. He also got a turpentine enema. The attendant was directed to try to keep him from sleeping. About nine o'clock, p.m., Dr. Popham ordered him to get one drop of a solution of three grains of the extract of belladonna to the ounce of water. This dose was repeated three times at intervals of two hours. At three o'clock,

a.m., on the following morning the pupils dilated a little, and he passed water freely. His bowels, which had not been disturbed by the first enema, which was retained, were now well moved, after the exhibition of a small assafetida enema. From this time he continued to improve without showing any other bad symptom. He was discharged on the 23rd, cured.

A Case of Fracture of the Pelvis, Complicated with Rupture of the Urethra, &c. By Stephen O'Sullivan, M.D., House Surgeon, Cork North Infirmary.

Patrick Hennessy, aged twenty, admitted to the North Infirmary on the 15th October, 1864, about half-past seven o'clock, p.m.

Was on an errand this evening delivering books, near the railway station, on the Lower-road, when a horse, drawing a heavy cart, took fright at the sounding of the railway engine whistle, ran away, and knocked the unfortunate young man down. The wheel of the cart passed over his body.

On his admission to hospital there are found several abrasions on the upper and lower extremities; the latter are apparently in a paralysed state, without anesthesia. The right limb appears somewhat everted, and quite powerless. Upon examination, however, there is found to be neither dislocation or fracture of the femur. He cannot bear to be disturbed, the least motion causing him much pain. He lies with most ease on his back.

The right testicle is forced up under that portion of the integument covering the *corpus spongiosum* of the penis. By a little manipulation the testicle is easily reduced to its proper position in the scrotum. Complains of much pain in the back. On examination, a puffy tumour is found over the right side of sacrum.

There are found no external marks of violence in front over the hypogastric region which would lead one to suppose the existence of any internal injury.

His pulse is scarcely perceptible at the wrist (120). Countenance very pallid. Is in a condition nearly approaching to syncope.

Stimulants are administered, but without any apparent effect.

Half-past eight o'clock, p.m.—Complains of violent pain in the abdomen, which continues to increase. Upon inquiry it is found that he has not passed water for some time, and a catheter is gently introduced. The instrument having reached nearly the neck of the bladder, is found not to enter that viscus but to take an unusual turn, and is consequently withdrawn, stained with blood, leaving no doubt of rupture having taken place.

From this time the patient continued to sink rapidly. Death took place at ten o'clock, p.m.

Autopsy made October 17th.—Upon opening into the peritoneal cavity, there is found an increased vascularity of the intestines, with one or two marks of ecchymosis. Some blood is found in the lower part of this cavity. The peritoneal covering of the bladder presents an appearance of extreme ecchymosis, which might almost, at first sight, be taken for gangrene. It is of a dark red colour. There is much blood effused beneath it, and on the surface of the bladder, as well as into the cellular tissue of neighbouring parts. Upon the introduction of a catheter (into the penis) it is found to enter the cavity of the pelvis, emerging from the urethra in front of the prostate gland.

The two horizontal rami of the *ossa pubis* are found fractured, and the symphysis is also partly disarticulated and crushed.

A large spiculum of bone from the back part of the symphysis pubis is found to project against the bladder.

There was much obscurity in the diagnosis of the foregoing case, and not without reason, when it is considered that the existence of so much internal mischief was not manifested by any corresponding external mark of violence. The only way in which it might be suspected that there existed fracture of the pelvis was by remembering that the urinary organs had sustained great injury, by a careful observation of the state of the lower extremities, and by noticing the position of the patient.

On Progressive Motor Ataxy. By Dr. L. W. SALOMONSEN. Translated from the *Bibliothek for Læger* for January, 1863. By WILLIAM DANIEL MOORE, M.D., Dub., M.R.I.A.; Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Examiner in Materia Medica and Medical Jurisprudence in the Queen's University in Ireland.

(*Continued from* Vol. xxxviii., p. 502.)

FEARING to weary the reader by the quotation of too many cases, I shall not here bring forward examples of the many irregularities in its course which this disease, like all others, may present, and of which numerous descriptions are to be met with. I shall only direct attention to the fact that it is especially the first stage which may be irregularly developed, or the symptoms of which may be partly, or wholly, wanting, and not present themselves until later in the course of the disease. The characteristic pains are most rarely absent; but Duchenne records a case in which they did not set in until more than two years after the commencement of the disease. More frequently the anomalies of the eyes, the amaurosis, and the paralysis of one or more of the nerves connected with the eyes, may

be absent in the commencement, but they, too, will always occur at a later period, though it may be only transiently. The same is true of other nervous affections. Only once has Duchenne found the sexual functions wholly unimpaired, although the disease was already in its third stage; and in like manner he has only once seen the abnormities of urination and defecation limited to slight constipation, although in this instance, too, the ataxia was far advanced. Most rarely of all does the disease begin from the first solely with signs of deficiency of co-ordination of movements; rather more frequently is it the case, on the other hand, that these begin in the arms, and do not, until subsequently, attack the legs. Lastly, there are also examples of the disease occurring hemiplegically, being confined to the arm and leg on the same side of the body; both Duchenne and Moreau (de Tours)[a] have recorded such cases. Not only does the disease thus acquire a quite peculiar stamp, but it is evident that it may thus more readily simulate, and be confounded with cerebral affections, especially with apoplexy, and that the treatment adopted may, consequently, be influenced thereby.

The same remarks which I made above with respect to the symptomatology may here be repeated in speaking of the diagnosis of ataxia, and especially of the distinctive marks between it and the other more or less allied nervous affections, which the researches of modern times have first more accurately classified and defined; and I may here especially refer to the article in the *Ugeskrift*. The observations which I shall now make are, therefore, very few. In the first period the signs are, separately, of course, usually rather uncertain; but when combined they acquire a pathognomonic value, and predict the subsequent development of the disease. Every spontaneous strabismus or diplopia must thus direct attention to the possibility of the presence of the disease; and, even if it is removed by one remedy or another, our suspicions ought not to be lulled: it will often return again, or the other symptoms may be developed without it. Still more does this apply where the strabismus is accompanied by complete or incomplete amaurosis. Each affection separately is, of course, of less importance; combined, they are much more serious. Usually only one eye is affected, and precisely for that reason the patient may easily fail to attach much importance to the symptom, or he may even himself be unconscious of its existence; too often, however, both eyes are attacked, and sight is completely lost, and that even early in the course of the disease—as, for example, in Charcot's and Vulpian's patient. Affections of the genital organs have about the same importance. If, without demonstrable cause, spermatorrhea, or impotence, should occur in a man still rather young, ataxia ought always to be be present to the physician's mind. Thus, for example, Duchenne has

[a] See l'Union médicale, 1862, No. 122.

seen a case where the patient, a man of twenty-eight years of age, consulted him for impotence, which had commenced a year previously; his look, however, struck Duchenne as remarkable; and, on closer inquiry, he found that he had, during the last two or three years, occasionally seen objects double: on the other hand, he nowhere felt pain. Now, were the impotence and the disturbance of vision accidental combinations, and was each, by itself, a distinct disease, or were they the precursors of ataxia? The future was soon to reveal this; for, in a very short time after the patient came under treatment, he complained that he could not securely balance himself, and soon after the ataxia was very well marked. Still, the characteristic pains are especially important in the diagnosis. Hitherto they have usually been regarded as neuralgic or rheumatic, and have been treated accordingly; now they will, especially when combined with one or more of the above described affections, naturally acquire a much greater importance. Trousseau goes on this point so far as to say that he has never yet seen them in any one who did not subsequently become ataxic. If the anomalies of movement have first occurred the diagnosis is, of course, easier; but even then it may be worth while to attend to particular circumstances. Thus the want of co-ordinating power need not, especially in the commencement, be very striking; and the lesions of sensibility may, even during the whole time, be altogether wanting. It is then chiefly the state of the movements, when they are unaided by sight, which can be a guide to the physician. Even Romberg lays particular stress on this point. He says:—"The patient must, even from the commencement of the disease, be able to see his movements, if they are not to be still more uncertain than under other circumstances. If he be made, while he stands erect, to close his eyes, he begins forthwith to tremble and shake, just as he walks and stands with more uncertainty so soon as it becomes somewhat dark. Thus a patient, whose sight was unimpaired, complained that he found it impossible to stand and wash in his dark sleeping-room; to avoid falling about, he was obliged to dress in a better lighted apartment. Another was obliged, when he went out in winter at six o'clock in the morning, to have a guide both at home and in the street." This symptom Romberg considers to be pathognomonic, for it is met with neither in other paralyses nor in uncomplicated amblyopia; and he therefore called attention to it so long as twenty-five years ago. Duchenne does not, indeed, lay so much stress upon it, but he shows decidedly that the lesions of co-ordination, although they are always present when the patient can see, become particularly distinct in the dark, or when the patient is not allowed to look before him; and he employs this circumstance especially to distinguish ataxia from the disease which, on less accurate investigation, it most nearly resembles—namely, paralysis of the muscular sensibility. Patients with the latter disease, in fact, provided they can see,

do not exhibit the least want of co-ordination in their movements, but perform them in a perfectly normal manner. Trousseau's practical sense has, on the contrary, led him independently to comprehend and estimate the importance of this fact. In the normal state, he says, persons can both stand erect and walk with closed eyes; but their gait is more insecure than when they walk with their eyes open. No one can walk quite as well with closed eyes; and even if one was assured beforehand that he would meet with no obstruction his steps would become unequal, and he would walk in a quiet, cautious manner. This is the case equally in the most normal condition, and in the purest paraplegia or in ataxia: it proves that even in the most healthy the sight is necessary to control the steps, and to make them even and uniform; and this is the reason why a blind man, long as he may have been blind, must always use a stick in walking, otherwise he would, in fact, constantly miss his direction. Now, he, indeed, incessantly loses it, but the stick brings him quickly back to the right direction. It is, therefore, not in this way that ataxia can be recognized. On the contrary, it may, even at a very early period, be distinguished from every other affection by the following marks:—If we desire an ataxic person to stand erect, with his feet close and parallel to one another, he does it only with a certain difficulty, even if his eyes are open and the disease only commencing. But, if any disturbances in walking have already occurred, it is quite impossible for him to hold himself erect when his eyes are closed; he immediately begins to tremble, and would fall about if he did not either open his eyes and lean on something, or make a great effort to recover his equilibrium. This is pathognomonic, for it is met with neither in the sound, nor in those who are hemiplegic or paraplegic, for example, after apoplexy, in general paralysis, nor, lastly, in the blind. If the disease is a little more advanced the same test may be employed in walking. Even if the patient, when he has leave to look, usually walks without tottering, maintains his balance perfectly, and, for example, complains only of feeling his limbs weak, he will, the moment he closes his eyes, or finds himself in the dark, begin to reel like a drunken man, and he will scarcely be able to walk a step without falling. On the whole, the patient's condition in walking is still less peculiar than that just described as existing when he is in the standing position; for the ataxic gait may, as the examples already quoted will have proved, vary very much: even if the patients are, moreover, perfectly blind their gait need not differ essentially from that of other amaurotics. Some walk slowly; others, on the contrary, quickly, with jerking steps, or they can take only short steps, or, finally, they walk quite stiffly, almost without bending the joints of the legs; but they all reel when they stop, often even if their sight is quite unobstructed, constantly on the contrary, so soon as they are unable to see. The second characteristic mark of the disease I am describing is the preservation, in all its stages, of the

muscular power, which is recognized partly by attempts to perform movements with the limb against the patient's will—partly by the dynamometer—partly, lastly, by investigating what exertion of force his apparently paralytic limbs, which he also himself feels to be weakened, are capable of exercising. Even if the patients cannot walk a step without support, it is often quite impossible to flex or extend the leg against their will; or they can, when sitting, themselves extend it so violently that another person standing opposite to them is thrown down by the blow; and as to the arms, Trousseau has seen patients who, if they only leaned upon something, could carry upwards of ten stone erect upon their shoulder; nay, he has even seen an old man of eighty who could, with ease, lift and carry his physician. The third mark which distinguishes this disease, particularly, from chorea, paralysis agitans, or the other kinds of more or less universal tremors, is, that the irregular movements in ataxia are produced only when the individual desires to perform a movement, while, in the other diseases, they exist also when the patient keeps himself quiet. Lastly, the fourth mark which distinguishes ataxia especially from diseases of the cerebellum is, that the patients affected with such cerebral diseases easily lose the balance of the whole body, and, therefore, reel much in walking and easily fall; while the ataxic patients' gait is difficult only because they cannot direct the movements of their limbs according to their own will. The patients with cerebral disease, on the contrary, do not so readily fall about when they shut their eyes, while they stand erect, as ataxic patients. Frequent vomiting will, moreover, usually accompany the cerebral disease; and all the precursors of ataxy, especially the pains, will be completely wanting.

As to its duration and course, we need scarcely observe that this is, in general, a chronic disease, often even one of the most protracted—that it may last ten, fifteen, twenty years without attaining any great development, even without passing its first stage, and without essentially disturbing the general health. On the other hand, it may, however, also be tolerably rapid: thus it may, for example, even in six months from the commencement, have become general; and the patient is, in such a case, usually so much reduced that the slightest intercurrent disease suffices to carry him off.

Of its etiological elements some are more, others less, well known. All writers agree that it is especially the age of manly vigour—from twenty to forty years—which is most liable to be attacked by it. Women much more rarely suffer from it. Romberg states that their number, in comparison to that of males, is only one-eighth; Duchenne has seen it only three times, Trousseau, among fifty cases, has met with it only four times in women. Still, Charcot's, Moreau's, and Isnard's three patients were all women. Trousseau's example of a man, aged eighty, suffering from it, and Isnard's patient of seventy-two years, show that old age is

not absolutely exempt from it. Onanism, spermatorrhea, syphilis, excesses of various kinds, the suppression of habitual perspiration of the feet, are, further, more or less correctly mentioned by all as the more proximate causes. One element there is, to which Trousseau is the only one to direct attention, and that certainly correctly—namely, hereditariness. This is not to be understood as if this disease—like phthisis, for example—passed as a direct inheritance from father to son; attention has too recently been turned to the point to admit of its having been as yet investigated. What I wish to say is, that the same is true of this as of other nervous affections—that allied forms are met with in several members of the same family—that the whole kindred in general are predisposed to nervous diseases. The latter, as is well known, often change their form; and, nevertheless, we are justified in referring them to the same class, and in recognizing a common bond of connexion between them. Even Isnard's patient will have justified this assertion; the examples quoted by Trousseau establish its correctness still more distinctly. The father of one of the ataxic patients, in his sixty-fourth year, became melancholic, but recovered completely. His sister was an idiot, and of her two children one was paralytic and demented, the other was an idiot. His father's sister was also insane, and one of her sons was epileptic. Another of Trousseau's patients had a brother, who was also ataxic; another brother who was attacked with hemiplegia; while one of his uncles and an aunt were insane. Lastly, this writer mentions a third patient, who had been for more than twenty years ataxic, but, like the others, had never suffered from any intellectual disturbance. His father killed himself; one of his brothers was clear in his intellect, but, under the influence of an irresistible impulse, uttered loud cries; the other brother suffered for many years from singular muscular spasms. These examples of the transmutation of nervous diseases from one form to the other are certainly as striking as possible; and there can be no doubt that the future will exhibit, still more fully, the correctness of Trousseau's opinion in this respect.

Before proceeding to speak of the most interesting point in this disease—namely, the question of its essential nature, pathological anatomy, and physiological pathology—I shall here dwell a little upon its treatment, not because it is one of the diseases in which treatment can accomplish much, but because the hopelessness of earlier writers has been in part shaken, and because the progress of science may be traced in this direction also. Against Romberg's disconsolate assertion, that "every patient suffering from this disease is irrevocably condemned; that every hope of recovery has disappeared, and that the physician ought to confine himself to being as little active as possible; and that he should, in fact, endeavour only to allay the pains by means of veratria ointment, and to regulate the digestion by cold lavements, avoiding evacuations of blood,

derivatives, long continued bath-cures, &c.," even Duchenne goes a little less far in a negative direction. If the disease is fully developed he doubts that any medical treatment can avail; but he hopes that when, hereafter, it can be recognized with certainty in its first stage, it may be arrested in its progress; and he knows undoubtedly, from his own experience, that at least some of its symptoms can be removed, or essentially alleviated, in the beginning, and even later in its course. It is especially local electricity, faradisation, which he has found of essential service. It has removed the strabismus, or the diplopia; has diminished, or temporarily arrested, the violent pains; and has even subsequently essentially lessened the cutaneous or muscular anesthesia, and has, at the same time, again contributed much to remedy the want of co-ordination of the movements, and vastly to improve the patient's gait. But to act upon the source of the disease—at that time (the date of the publication of his essay) still unknown to him—he considers all local means insufficient: a more general treatment is, for that purpose, necessary. If there be a trace of previous, or of still-existing syphilis, mercury or iodine is, of course, indicated; but, even where this definite indication is wanting, Duchenne has seen benefit from the preparations of iodine, and he therefore always combines local faradisation with their use. Of course this treatment is purely empirical, and he consequently states, beforehand, that he has not so much faith in it as to prevent him changing it for any other mode which others may find efficacious. Trousseau, on the whole, agrees with Duchenne, except that he proposes the use of opium, or of belladonna to relieve the pains, of a compressor of the prostate in spermatorrhea, and of turpentine in retention of urine. While he enforces prudence in the use of sulphur-baths, or of the water-cure, he does not deny that these means may sometimes be of use. In one case, treated by Bourguignon,[a] the water treatment, together with electricity, and the internal use of strychnia, really appeared to have cured the disease. In 1861, however, Wunderlich proposed a new remedy—namely, nitrate of silver—in this disease. He was led to try it by the fact that a lady, who suffered from extremely violent hysterical spasms, followed by universal paralysis, both of sensation and motion, and who knew, from previous experience, that this paralysis was rapidly removed by the use of this remedy, was not in the least improved by other medicines which he tried, notwithstanding that he had led her to believe that they contained the salt of silver, while she got rid of her paralysis as soon as he put her upon that preparation. In the course of a short time five cases of well-marked, though not very advanced, ataxia came under his care; in them all he tried this remedy almost exclusively, and in all it had an excellent effect. All the ataxic symptoms were essentially relieved; ordinate, regular movements took place; the gait became

[a] See L'Union médicale, 1862, No. 39.

more steady, so that the patients could take long walks, and, without difficulty, go up and down stairs; the pains diminished considerably; the anesthetic signs were much lessened, or ceased altogether; defecation and urination again became normal; and, in some cases, the previously-extinct sexual impulse even returned; at the same time the weight of the body steadily increased, and the patients' whole condition and appearance improved. He gave it in pills, containing from $\frac{1}{20}$ to $\frac{1}{8}$ of a grain in each, increasing from three to six times a day; and, on the whole, the patients got a considerable quantity, namely, from half a drachm to a drachm. Later, he has twice had occasion to try this remedy,[a] and he has likewise seen striking benefit from it. He admits that its effect can be explained neither physically nor chemically; that it is pure empiricism, or induction, which has led him to employ it. He further remarks that the improvement attained is not necessarily lasting, but may, perhaps, prove to be only transitory. But he asserts, and correctly, that in so formidable a disease, and one hitherto believed to be incurable, even a temporary improvement must be considered a benefit. He does not suppose that he can, by this means, remove the changes in the substance of the spinal marrow, which are met with on every dissection of a far-advanced ataxic patient, and he therefore feels confident that the remedy will prove inefficacious in cases where such changes have taken place. But, on the one hand, he believes that the ataxic phenomena may occur without the spinal marrow being necessarily perceptibly altered on anatomical investigation; and, on the other, he does not yet know whether the abnormities in the spinal cord—even if we admit that they are met with in all cases—are the cause, or a secondary consequence of the disease; and, on these suppositions, we may hope that if we restore the peripheral nervous functions we may also remove the central lesion.

After him several have adopted this remedy. Trousseau, who in his theoretical views on this point partly resembles him, has not yet, indeed, had the opportunity of testing it with sufficient steadiness in this disease; but he has so often seen marked benefit from its use in other neuroses, especially hooping-cough, epilepsy, and hysteria, that he *à priori* places great confidence it, and warmly recommends its employment. But it has been advantageously prescribed, by other French writers, at least in three cases. Thus, Charcot and Vulpian, in the article quoted above, mention that in two patients they have with it obtained very essential improvement, both in movement and sensation; that the pains have ceased, and that in one case even the sight became better; subsequently they must have successfully used it several times, for quite recently the Academie de Médecine has awarded[b] them a prize of 1,000 francs for a

[a] See Archiv der Heilkunde, 1863. Heft 1, p. 43.
[b] See L'Union médicale, 1862, No. 146.

memoir sent in, upon the use of nitrate of silver in ataxia. This prize has been given them from the Barbier legacy, "which is to be distributed to those who have discovered perfect remedies for any disease hitherto considered incurable, or a remedy which nearly approaches thereto." Theoretically they, moreover, start from the principle, that material changes in the spinal marrow lie, from the commencement, at the bottom of the disease, and produce its phenomena; but they believe that when it is possible by this remedy to arrest and to remove the morbid process, the altered nerves may be again restored, and their functions more or less completely re-established. They rest this view upon the fact that the dissection, in their patient described above, among other things exhibited regenerated nerve-fibre, inasmuch as it shows that such a restoration is possible, even in a far-advanced stage; it is therefore much more likely to take place when in the commencement of the disease the anatomical changes are much slighter. Lastly, Moreau, too, has, upon their recommendation, employed the medicine in his patient, and also with excellent effect. In this case the disease had progressed for four years in spite of all the means employed. Under the use of nitrate of silver all the symptoms diminished to such a degree that the patient thought herself cured, and that, in fact, every medical man must consider her state to have been much improved. It was not only the bodily symptoms which were in part removed, but the epileptic fits, with which she had before very often been attacked, and which certainly must rather be looked upon as evidence that the disease had extended to the medulla oblongata or cerebellum than as an accidental complication, remained away all that time, and her weakened faculties, especially memory, improved. In the middle of the treatment Moreau gave her, as a test, without her knowledge, for fourteen days, pills consisting only of crumbs of bread; the improvement which had already begun ceased, and her state became immediately aggravated. Directly after the nitrate of silver was resumed, on the contrary, the improvement again went on rapidly. As a proof of the impregnation of the system by the medicine, he calls attention to the fact that not only does the skin often become of a leaden colour, but that the gums also readily become bluish, just as in lead poisoning; a great part of the salt of silver, too, is excreted, as Cloez has shown, even in a short time after the commencement of the treatment, in the urine.

After these purely practical observations, we shall proceed to discuss the more theoretical questions which our subject gives rise to; and, in the first place, especially as to the essential nature and pathological anatomy of the disease. Duchenne has sketched the disease only from his clinical experience, derived chiefly from private practice; and, in 1861, he had as yet seen only one dissection of a patient affected with it; this gave a completely negative result. Both the whole of the brain and the spinal cord appeared (it is true, they were not examined

microscopically) perfectly healthy. Nevertheless, he does not doubt that some change or other, certainly material, must be at the bottom of the disease: and this he is most inclined to seek in the cerebellum or the surrounding parts. The power of co-ordinating the more complex muscular actions, which walking, maintaining the standing position, using the arms, and every voluntary movement require, is, in fact, he says, of a psychical nature, and physiological experiments have long since referred it to the cerebellum. Rolando, in 1809, after numerous experiments, arrived at the conclusion, that the cerebellum was the source and centre of all movements. Flourens, in 1822, somewhat modified this view, as he considered this part of the nervous system to be the seat only of the capacity of co-ordinating all the voluntary movements, while he believed that the latter proceeded from other parts of the central nervous system. A little later, in 1828, Bouillaud still further modified this theory, stating that the cerebellum is the nervous centre only for the regular co-ordinated performance of the several acts of locomotion, and for preserving the equilibrium of the body, while other movements, especially all those connected with speech, are co-ordinated by the anterior lobes of the brain. This view, which he has subsequently adhered to, both in his Nosography (1847) and in his numerous lectures, he had arrived at partly from vivisections, in which he irritated, cauterized, or removed portions of the cerebellum—partly from clinical observations, especially of hemorrhage in that organ—and he long since designated the disturbance of the power of motion so produced by the term "ataxy." The animals operated on were often affected also with amblyopia, or amaurosis, with or without strabismus; and this Bouillaud explained by the fact that the tubercula quadrigemina lie so near the cerebellum that they are liable to be injured along with it, or that, at all events, the irritation is easily propagated from it to them. The same is true, adds Duchenne, of the pedunculi cerebelli, injury of which produces violent pains about the body, and in some cases spasms. Judging from the course of the disease, Duchenne, therefore, believes that it in general begins by attacking the tubercula quadrigemina, the optic nerve, and the motor-nerves of the eyes—that it proceeds thence backwards to some part of the pedunculi cerebelli, and finally reaches the cerebellum. Of the anatomico-pathological lesion of the optic nerve he has, in several cases, been able to satisfy himself ophthalmoscopically during the patient's life—having thus succeeded in demonstrating the existence of atrophy of the optic disc, which certainly indicates a lesion of the nerve at its origin or in its course. Lesion of the other parts of the brain, on the contrary, as I have already said, he has never yet had the opportunity of observing, and he, therefore, assumes them only by the way of induction. His expectations were, however, soon to be disappointed; as it has since appeared that it is quite a different part of the central nervous system which is affected in this disease. Thus, in April,

1861, Bourdon[a] had the opportunity of making a *post mortem* examination of an ataxic patient, who died of an intercurrent gastric attack. The disease had lasted about six years, but had not, until within the last six months, attained to any great height. The motor anomalies had continued limited to the lower extremities; the upper extremities were completely normal up to the time of death, and sensation was everywhere quite unimpaired. In the left eye there were diverging strabismus, ptosis of the upper eyelid, and considerable dilatation of the pupil; the patient had become myopic, and there was occasional diplopia. The dissection was performed with the assistance of an experienced micrographer, Luys. With the exception of slight congestion in the grey substance, especially in the thalami and corpus striatum, nothing abnormal was met with in either the brain or the cerebellum. Both oculo-motor nerves and, although in a less degree, both abducent nerves were, on the contrary, found reduced to scarcely half their ordinary thickness, were very friable, and were of a grey colour; of the nerve fibres little more than the sheath remained. In the spinal cord, on the other hand, very considerable abnormities were discovered. All the membranes were, especially inferiorly, highly congested. The posterior columns were, from the tuberculum brachiale, gradually increasing downwards, and most strongly marked in the lumbar region, changed to transparent vitreous filaments—in some places of an amber yellow, in others, of a yellowish red colour. Their consistence was less than normal, but they were not diffluent, nor was their continuity interrupted. This change was due to the nerve fibres themselves; for most of them had, considered as anatomical elements, wholly disappeared, only the empty nerve sheath remaining. In the fibres, of which something yet remained, the cylinder-axis was, indeed, still perceptible; but it was uneven, tomentose, and rather of an amber-yellow. The nerve-tissue was intersected by a great number of capillary vessels. The grey substance had, likewise, from the lumbar region downwards, lost its consistence; many of its nerve-fibres were broken, and of the nerve-cells only some were preserved—most of them were collapsed, ramified at their edges, and covered with many more pigment granulations than normal; they were, in a word, in a state of involution. Deposits of hematosin, evidences of previous congestions, were also met with; all the capillary vessels, too, were here highly loaded. On the contrary, the anterior and lateral columns of the cord were perfectly normal. All the nerve-roots, forming the cauda equina, were flat, ribbon-shaped, greyish or yellowish, transparent, less bulky than normal. This was true particularly of all the posterior roots; and here the microscope also exhibited completely the same changes as in the posterior columns. The anterior roots, on the other hand, were much

[a] Archives générales de médecine. November, 1861, and April, 1862.

less affected; they were essentially only slightly atrophic; the nerve-fibres in them, in particular, were perfectly preserved, with all their elements. Lastly, the ganglia of all the lumbar nerves were altered about in the same manner as the grey substance; they were very red and vascular—their sheath was thickened—their ganglionic cells were in part corrugated, and were abundantly covered with reddish-yellow pigment, though some of them had still retained their normal relation to the surrounding nerve-fibres.

In this case, therefore, a considerable material lesion was demonstrated, not, as Duchenne had been most inclined to assume, in the brain, but in the spinal cord, and there, moreover, not in the anterior, but in the posterior columns and roots, and almost exclusively in these. The question, then, was, whether this lesion could really be regarded as characteristic of this disease, although it was at first so unexpectedly met with, and did not appear capable of giving the key to its explanation, or whether it was only an accidental complication in an ataxic patient; on this point only further experience could justify any opinion. But neither was this wanting. In a short time after the publication of Bourdon's essay, Dumenil, of Rouen,[a] communicated a perfectly corresponding dissection of an ataxic patient. Soon after, Oulmont[b] had the opportunity of examining a similar patient, who, after having perceptibly improved under the use of sulphur baths and various tonics, was attacked by bronchitis, and died in a few days with supervening cerebral symptoms. Even in this case there was in the brain only slight injection of the pia mater. On the other hand, Oulmont, likewise, assisted by Luys, found precisely the same changes in the posterior columns of the spinal cord as were met with in Bourdon's case, except that they were less advanced; in addition, he found the connective tissue of the cord hypertrophied, and a great number of amyloid bodies were seen scattered about in the posterior horn of the grey substance. Bourdon was now convinced, particularly in consequence of the agreement of the results of these dissections with those which he had found mentioned by earlier writers, and which I shall just now describe, that the anatomico-pathological expression for ataxy is a lesion of these parts of the spinal cord. Apart from less essential changes, it may, he says,[c] on the whole, be designated as a degeneration, combined with atrophy, of both the nerve-tubes and the nerve-cells; and he then endeavoured, starting from this point, to reconcile this discovery with our physiological knowledge, and thereby to explain the mechanism of the disease. A few weeks later, Charcot and Vulpian, with more knowledge of the subject and more authority, but on

[a] See L'Union médicale, 1862, No. 17.
[b] See L'Union médicale, 1862, No. 41.
[c] Archives générales, April, 1862.

the self same principle, endeavoured to establish a more elegant theory of the disease. The dissection of their patient, in fact, yielded the same results as the two above mentioned. Here, also, the posterior spinal columns, especially from the tuberculum cervicale downwards, were found to be greyish through their whole thickness, with only here and there slender streaks of intermingled healthy tissue; they were semitransparent and slightly softened. Most of the nerve-tubes had here disappeared; only very few healthy ones were still seen scattered through the midst of the fibrillar stroma, which seemed to be formed of connective tissue, principally, no doubt, the empty nerve-sheaths, but was somewhat hypertrophied; only in the white streaks were the nerve-tubes healthy. Numerous partly ramified congested vessels were met with, their walls everywhere covered partly with fat, partly with granular, partly, lastly, with amyloid bodies; these were present also, in great number, around or in the nervous tissue.

The anterior and lateral columns were perfectly normal; so was the grey substance, except, perhaps, in the apex of the posterior horn, where some amyloid bodies were seen. Of the nerve-roots the anterior were quite healthy; the posterior, on the contrary, were all, from the last cervical nerve downwards, about of the same grey colour as the posterior columns, and were so reduced in volume that they were much slighter than the corresponding anterior roots; in them, too, however, some healthy white streaks were seen. Under the microscope, also, only very few healthy nerve-fibres were found in them; all the others were atrophied, almost as delicate as the normal cerebral fibres, and they had a somewhat varicose appearance; they were exceedingly like nerve-filaments which are formed afresh after a nerve is divided. These changes were all most marked in the dorsal region. The medulla oblongata and brain were perfectly healthy. Both optic nerves were on the contrary, from the optic disc, a little slighter than natural, and were of a greyish colour; and posteriorly, behind the decussation, they became progressively thinner and flatter; not a single healthy nerve-filament was found in them, only connective tissue covered with fat and hematosin. The thalami, corpora striata, corpora quadrigemina, and the pedunculi cerebri were, on the other hand, perfectly healthy.

This result was still clearer and more satisfactory than Bourdon's; the lesion was limited exclusively to the posterior columns and nerve-roots, and to the optic nerves; all the other parts, which in the former case were not wholly unaffected, and the changes of which might, therefore, modify the symptoms, were here perfectly sound; and, both in a clinical and anatomico-pathological point of view, this case may, therefore, be taken as typical.

In addition, two dissections have very recently been contributed; one

was performed by Marrotte, assisted by Luys.[a] The disease was of fifteen years' standing; still the sensibility was wholly uninjured; the mobility, on the contrary, both in the arms, but especially in the legs, was much affected. Acute pleurisy carried off the patient. The posterior columns in the dorsal and lumbar regions of the spinal cord and the posterior nerve-roots were altered precisely in the manner just described; the corresponding anterior parts were healthy Still the entire of the grey substance in these parts was at the same time somewhat affected; it was of a reddish colour, strongly hyperemic; the nerve-cells were not so distinct as normally, and were covered with abundant pigment. Amyloid bodies were here, too, scattered in great quantity among the normal elements. Not the least abnormity was met with in the sciatic nerve or in the muscles of the lower extremities. The other dissection was performed by Vigla, with Sappey's assistance.[b] The disease had lasted thirteen years; the sensibility was somewhat blunted, but had not become so until within the last few months; the mobility of the legs, but not of the arms, had, on the contrary, been for ten years so much affected that the patient could neither stand nor walk, but had to be always carried about. The eyes were not implicated. The patient died of phthisis. The brain was perfectly healthy. The spinal cord was altered only in its lumbar portion, the posterior columns being here somewhat atrophied and greyish; they were not, however, microscopically examined. The anterior nerve-roots proceeding from this part were quite normal; the posterior roots, on the contrary, were so atrophic that, on comparison with the corresponding roots from another healthy spinal cord, they appeared to have lost from two-thirds to three-fourths of their volume; they were flat, ribbon-shaped, and were of a greyish-red colour. Only a few of their nerve-fibres were still normal; most of them were slender or varicose, had lost a portion of their contents, or in some the latter had in spots, or totally disappeared.

In view of all these concordant results, arrived at by various independent and competent observers, and which, according to a short statement by Bourdon, have been verified also in several cases by Friedreich, of Heidelberg, there can no longer be any doubt that the anatomico-pathological foundation of ataxy has been discovered, and that this affection has, according to the general terminology, been transferred from the class of the neuroses, to which it was first referred, to constitute a link in the constantly increasing class of diseases in the central nervous system, where a material lesion is no longer merely suspected, but has been actually demonstrated. There are, indeed, not many who will doubt that the change in the spinal cord and in the posterior nerve-roots

[a] See L'Union médicale, 1862, No. 67.
[b] See Trousseau, l.c., p. 826.

is, in the proper sense of the word, the foundation of the disease; that it is the cause of the symptoms, and that it is present from the commencement; in other words, that no really ataxic phenomenon manifests itself before certain parts of the central nervous system have been affected, and that the abnormities, in their development, constantly keep pace with the progress of the disease. Both processes are in the highest sense chronic; both may for a long time continue stationary; nay, may even retrograde towards recovery. So far as the symptoms are concerned this is sufficiently established by many of the cases on record. As to the nervous lesion, Charcot's and Vulpian's dissection especially is in favour of this view, inasmuch as indications were met with that nerve-filaments were on the point of being reproduced. Placing these two processes in direct relation to one another is so completely justified by our positive knowledge of the relation between the anatomical foundation as cause and the morbid phenomena as effect in all other diseases, and must certainly occur to every one as so natural and rational that it would be superfluous here specially to put forward this view, if such a course had not been definitely called for. This is rendered necessary by the fact that Trousseau, who, from the first, has taken a great interest in this disease, and has accurately traced all its phases, has in this respect put forward an opposite interpretation. He, of course, no longer doubts that the ataxic symptoms and the changes in the nervous system are mutually connected; but he nevertheless continues to refer the ataxy to the neuroses, because he considers the lesion of the spinal cord to be not primary, but secondary, as an effect of a hitherto unknown general condition of the system; and this he assumes as the basis of the disease. How should we otherwise, he says, be able to explain the fact, that parts which are so little connected with one another as the posterior columns of the spinal cord, the motor nerves of the eye, and the optic nerves, should be attacked simultaneously or successively, but always in one and the same manner? In all the pathological development is, in fact, the same. There is from the commencement hyperemia, which subsequently produces atrophy and degeneration. Is it not, then, most natural to assume that the unknown cause producing the ataxy fixes its manifestations, which always remain the same, on different, but mutually connected, parts of the central nervous system; and have we not something analogous to this in the mode in which the other unknown causes, producing hysteria or cachexia exophthalmica, act? Do we not in these diseases also frequently find hyperemia and congestion? Does not the last-named affection in particular often give rise to material changes in the heart, the thyroid gland, and the retina? Seen from an ideal, or, if we will, from an antiquated point of view, there may certainly be some justification for this line of argument, but only on the condition that it be extended much farther than is here indicated. With the same right

by which Trousseau assumes an unknown general cause of the disease in question, he might, it seems to me, assume a similar one in every disease whatever; hepatization of the lung, for example, then becomes only the secondary expression of a general blood disease; and we are not more justified in deducing the pneumonic symptoms from it than in placing the ataxic in connexion with the nervous affections. Trousseau's well-known vitalism here exhibits itself in an extreme degree. In his opposition to organic cysts or to localizers he goes so far that, in spite of the opposition as yet recently raised in the Academy, especially by Bouillaud, he continues to refer cachexia exophthalmica to the neuroses, and now endeavours to do the same with regard to ataxy. It would, in my opinion, be a retrograde movement to follow him in this course; not only for the promotion of science, but also in a purely practical point of view, it is more correct to start from actual data than to seek the mysterious; and, as specially concerns ataxy, its cause appears to me now to be so clear, and so definitely indicated, that I can only wish we had advanced as far with respect to the nature of many other nervous diseases—for example, epilepsy.

It was not until we had the information derived from the dissecting table in our possession that we were fully justified in establishing ataxy as a morbid unity. So long as we had before us only the symptomatological picture our right to do so must always have remained more or less doubtful; the most important phenomenon, the want of co-ordination of movements, might, in fact, according to the opinion of many, for example, of Bourdon, manifest itself also in totally different diseases, for example, in hysteria, chlorosis, poisoning by lead or alcohol; now, since not only all the various symptoms and their sequence, but also their material foundation, have been demonstrated, doubt no longer exists upon the point. But at the same time that we admit this, we must also feel convinced that it is in reality not a new disease which has been discovered and received into nosology; that there can be no comparison in this respect with the discoveries of Bright, Addison, or Virchow, but that we have only a more accurate and more complete development of what we already possessed. To show the correctness of this view, which I have already put forward in the commencement of this article, I may be permitted briefly to quote some cases from earlier writers, which Bourdon in particular has collected.

Cruveilhier has three cases in point. The first is that of a woman, aged fifty-three; the disease was of one year's standing, and manifested itself by violent pains in the lower limbs, loss of sensation in the whole of the lower part of the body, completely normal movements of the legs in the recumbent position, but great tottering and uncertainty during walking; incontinence of urine and feces. On dissection the brain was found to be normal. In the dorsal and lumbar portions of the spinal cord the

posterior columns, but only these, were found changed into a yellowish grey, semi-transparent substance; the anterior and lateral columns, as well as the grey substance, were quite healthy. In the second case the disease was of two years' standing; both the arms, and especially the legs, were affected; the sensibility was much weakened; all the movements, though feeble, were still possible in the horizontal position; the standing position or walking, on the contrary, were quite impossible. Urine and feces passed involuntarily. Dissection exhibited a grey degeneration of the posterior columns of the spinal cord and atrophy of the posterior nerve-roots. In the third case the history states merely that a young amaurotic girl, whose sensibility was unimpared, but who in point of motion was paraplegic, died of an intercurrent disease. The optic nerves were found atrophied both before and behind the decussation. The posterior columns of the spinal cord were throughout the whole of their length of a greyish-red colour; only here and there were some white filaments met with; all the other parts of the spinal cord, as well as the brain, were quite healthy.

Ollivier gives two cases. One is that of a man, aged forty-eight, whose illness was of thirteen years' standing. It had commenced with pains in the lower extremities; soon after walking became uncertain; he could no longer direct his legs; the one knocked against the other, and in consequence he tottered. Shortly after the arms were similarly attacked. Sensation was much blunted. Vision diminished, and before death he became almost totally blind. Some symptoms of paralysis of the rectum and bladder also existed. The patient died of cystitis, on which peripneumonia had supervened. On dissection the posterior median fissure was found to be of a dark grey colour, and the posterior roots of the nerves of the cauda equina were small, greyish, and atrophied. All the other parts of the central nervous system and the anterior nerve roots were, on the contrary, healthy. The second case is that of a man, aged forty-two, whose illness had commenced five years previously with tingling in the fingers, and had gradually extended first to the upper extremities and so to the trunk and legs; the tingling had then been gradually replaced by incomplete paralysis. All his movements were unsteady and jerking, tottering and slow. His sensibility was blunted. He died of an attack of pneumonia; for the last week there was retention of urine and feces. Quite the same changes were met with in this as in the preceding case, only the degeneration in this instance was more developed above, agreeing with the fact that, contrary to the usual course, the disease had begun in the arms.

Finally, Romberg, too, adduces examples of the same. A provincial physician had, twelve years previously, been attacked with paresis of the legs and with amblyopia; the latter subsequently passed gradually into complete amaurosis, and all the signs of tabes dorsalis were

developed; the sensibility of the skin and the temperature nevertheless continued until shortly before death. Romberg and Froriep performed the dissection, and were not a little surprised to find atrophy of the spinal cord to such an extent that the latter had lost one-third of its volume; and also to observe that the atrophy was strictly confined to the lower part of the posterior columns and nerve-roots, both of which had lost almost the whole of their medullary substance, and had become transparent and of a greyish yellow colour. The anterior columns and nerve-roots, on the contrary, were normal. Such pathological changes, he says, are, moreover, not unique, but are frequently met with in tabes dorsalis; the changes on which the characters of the disease depend may, in fact, according to his opinion, vary somewhat, but we most frequently find partial atrophy of the spinal cord and of the nerve-roots, especially of the lumbar portion and of the posterior roots. Where amaurosis exists, there is almost always at the same time atrophy of the optic nerves and of the optic commissure, sometimes also of the thalami.

From these examples it is sufficiently clear, that with respect to the nature of this disease, writers have as often as otherwise moved in a circle—just as certainly as it may be assumed to have existed in all ages, has it been observed, followed in its progress, and long since studied in its anatomical relations; but the descriptions given have been forgotten or perhaps unknown; authors have wished to maintain the priority of modern times; and, though sufficient material for its elucidation lay before them in all directions, they have wished, from its symptoms alone, to form a new, hitherto unobserved, independent disease. Now, whether, since the mistake is established, we wish to call the disease ataxy or tabes dorsalis, or as Ollivier does, myelitis, is nearly indifferent. Myelitis is certainly the least suitable name, inasmuch as the disease does not present any inflammatory symptom whatever; tabes dorsalis has this against it, that it has been employed to designate various morbid conditions in the spinal cord, not a certain, definite, specific lesion; while the name ataxy, on the other hand, is confined to the symptomatology, and does not indicate the material lesion. Least of all ought it to be called Duchenne's disease; Duchenne has in fact only accurately traced and described the gradual development of the several symptoms, and has added but little that is new to the much that was previously known; but he has been in such a hurry to blazon forth his imagined discovery that he has not allowed himself time to await the decision which anatomico-pathological investigations have now brought about, and which must necessarily quite alter his original views.

In conclusion, it remains to investigate the physiological basis of the pathological phenomena. The ophthalmic affections have their palpable cause in the abnormities in the corresponding nerves of the eyes demonstrated both during life and after death; the pains find their most

satisfactory explanation in the fact that an affection of the central nervous system is present, and the want of intellectual disturbance and the unimpaired maintenance of the voice and speech agree with the observation that this disease has its seat not in the brain, but in the spinal cord. There is, moreover, perfect harmony between the symptoms and the *post mortem* appearances, inasmuch as the former commence in general in the lower extremities and there attain their greatest development, while the latter usually show that the degeneration has commenced, and has made most progress in the lower part of the spinal cord and in the nerves proceeding therefrom; not only the phenomena presented by the extremities, but also the abnormities of the urino-genital system are thus satisfactorily explained. What, on the contrary, does not appear at first view to be in unison with our physiological experience is, that during the patient's life it is always chiefly the mobility which suffers, while dissection proves that it is exclusively the posterior columns and the posterior nerve-roots of the spinal cord, which are the seat of the degeneration. On reviewing the symptoms, we should, if we suppose it granted that the spinal cord is affected, rather have expected that the localization of the disease should be in the anterior columns and roots, which, on the contrary, are always normal; and if we were, from the *post mortem* appearances alone to draw a conclusion as to the anomalies presented during life, we should infer that sensation only, or at least chiefly, must have been affected; yet the latter invariably suffers much less than the mobility; it is never impaired unless secondarily, and sometimes, as, among others, Romberg's, Bourdon's, and Oulmont's cases prove, it escapes altogether. It is no analogy to the atrophy of the motor columns and roots in progressive paralysis which ataxy presents. Is it then possible to account for this circumstance, to bring the experience and deductions of physiology into agreement with the facts of pathology, or are we compelled with respect to this disease to give up the method which has of late years so largely increased our knowledge in many other directions, and to admit that we must here resign the guidance of physiology? No! for just at the time that the attention of pathologists has been directed to this disease, physiological vivisections have pointed out what was erroneous in many of our previous assumptions as to the functions of the several parts of the nervous system, and have in place of these established others, by the aid of which we are now in a position, to a certain degree, to solve this difficult problem.

After Bell and Magendie distinguished between the two different kinds of nerves, most perfectly represented by the two spinal nerve-roots, it was by all admitted, not only that each of these roots had its particular, definite function, but also that a corresponding relation existed between the different columns of the spinal cord itself. Just as the anterior root was assumed to be exclusively motor, the anterior columns were considered

to be destined exclusively to conduct the impulse of the will from the centre to the periphery; and as the posterior roots were supposed exclusively to convey the sensory impression from the periphery inwards to the spinal cord, the posterior columns were correspondingly believed to conduct it further to the seat of consciousness—the brain. The lateral columns, whence the nervus vagus, the accessory nerve of Willis, and other so-called respiratory nerves spring, were then supposed to rule the involuntary, especially the reflex and respiratory, movements. But all these propositions, which with the exception of the fundamental doctrine of the different function of the nerve-roots, were purely theoretical, and were based upon no practical foundation, have been completely shaken by the results of more modern experiments. That the function of the nerve-roots varies will remain to all ages unassailable; but the properties of these parts have shown themselves to be different from what had hitherto been supposed. The anterior roots are, indeed, exclusively motor; but that they are, therefore, themselves insensible, as was supposed, Bernard's discovery of recurrent sensibility has proved not to be the case. The posterior roots are, indeed, exclusively sensory; but when all influence upon motion has consequently been denied them, more recent experiments have shown that this does not hold good. And as to the spinal cord itself, still more completely altered views have become necessary. This is not the place to take the latter fully into consideration, our present subject renders it necessary only to mention what concerns the posterior columns and roots, and to this therefore our incursion within the precincts of physiology shall be confined.

The anterior root has importance only for motion, and is without any influence whatever upon sensation; after its division the parts to which it is distributed have always in all animals completely retained their sensibility, and are paralysed only with respect to motion. The converse, on the contrary, does not hold good with respect to the posterior roots, as Bernard in particular has pointed out.[a] By experiments, commenced so early as in the year 1842, but only lately concluded, he has proved that the parts whose sensibility has been destroyed in consequence of the division of the posterior roots, can still, indeed, perform all kinds of movements, though not in a perfectly normal manner. To understand the possibility of this state of things, we must remember that each muscle, in addition to nerves of motion, receives also nerves of sensation; these supply the muscles with a peculiar kind of sensibility, which Bell called the sixth or "muscular sense;" while Gerdy, and subsequently Landry, have termed it the "sensation of muscular activity," and Duchenne directly calls it the "muscular sensibility." It makes the

[a] Claude Bernard: Leçons sur la Physiologie et la Pathologie du Système Nerveux, T. i. Paris, 1858.

brain, and consequently the individual, acquainted at every given moment with the state of the muscle, whether it be at rest or in contraction, with the degree of the muscular activity, the amount of contraction, &c., and it thus becomes necessary to ensure to the more complex movements the indispensable co-ordination between the different muscles. We must, moreover, remember that every movement may be assumed to be preceded by a sensitive impression, an excitation. For the involuntary movements this probably proceeds from one or other point of the surface of the body, or of the internal organs; for the voluntary movements there is, on the contrary, only one definite inner excitement, and that is the will. It is the expression of a preceding perception, which is preserved as a remembrance; it is therefore only a peculiar form of sensibility, proceeding from the cerebral hemispheres, and which is not conducted through a continuous nerve-fibre to the contracting muscle, but first passes other parts of the spinal cord, certainly ministering to sensibility, before it reaches the motor nerve. This, then, finally sets the muscle in action, and the muscular contraction thus becomes the last link in the series of, and the final expression of a number of connected, but in their nature mutually different phenomena. Lastly, we must remember, that a simple excitation of a single little point of the skin or of the spinal cord does not produce contraction merely of a single muscle, but often gives rise to very complex and extensive movements. With this fact before our eyes, the assumption becomes altogether improbable that the sensory impression is conducted directly to one or more motor nerves, that a direct transition takes place from one kind of nerve to another; it is much more reasonable to assume that in the spinal cord there are many, special, mutually connected centres of motion, whence the motor nerves proceed to the several parts, and which thus determine their co-operation or antagonistic relations; we shall then, in fact, be easily able to understand that by their excitation a complex, but completely co-ordinated movement is produced. Even from these considerations it is clear that all motion is in a certain mode under the dominion of sensation; but this relation becomes still more evident when we study the results of vivisections.

The first fact by which Bernard's attention was directed to the possibility of the importance of the sensibility of a part of the body to its movements—a matter which had hitherto been wholly overlooked—was that when he divided the posterior lumbar nerve-roots on one side of the spinal cord in a frog, the animal bent up only the hind leg, whose sensibility was uninjured, against his fingers, which he held around the body of the frog, while the other hind leg, indeed, moved, but without being directed against any object. A healthy frog always bends up both legs and endeavours with them to push away the fingers and to liberate himself from them. If only sensation had been impaired in the above described operation, such a frog would do the same; but it never does

so. After Bernard's attention was directed to the point he extended his investigations further in the same direction, and soon ascertained the following:—If in a frog so operated on both hind legs be laid out alike on a flat surface, the animal voluntarily draws in only the sound leg again towards the body; the insensible leg it either keeps constantly extended, or, if it makes a movement to draw it back, the limb seems always to go out from the other, still sensitive leg. In leaping or swimming the frog therefore continues apparently to move both hind limbs equally well. If the posterior lumbar nerve-roots are divided on both sides, the same holds good for both hind legs. If they are extended the frog does not draw them back; they are in general not moved at all, unless a still sensitive part of the body—for example, the fore limbs—be irritated; if this be done, the insensible hind limbs, indeed, also take part in the reaction, but their movements are more uncertain than normally; they are convulsive, without object and without harmony. The swimming movements are irregular, and on land the animal creeps with the hind limbs and jumps only with difficulty. If he be held with the fingers around the body he makes no movements to free himself from them. So if the posterior roots be divided in all the four limbs the frog does not move at all spontaneously, but continues to lie immovable; and if he be excited, for example, by pricking the still sensitive head, he makes only unharmonized and irregular movements with the limbs; a combined movement of them, such as, for example, takes place in swimming, is not produced. If, lastly, all the nerves of both fore legs and one hind leg be divided, the other hind-leg being left untouched, we see the latter continue to execute perfectly normal movements; but if we now divide also the posterior roots of this leg without touching the anterior, its movements immediately become uncertain and un-co-ordinated; the animal spontaneously executes not the least movement; even if it be laid on its back in the water, it remains perfectly quiet in this abnormal position, while before, when the posterior roots were not divided, it voluntarily moved this leg with the evident object of turning itself round. All this does not proceed from anesthesia of the skin; for if only the skin be cut off all the four limbs the frog swims about for some time perfectly well; it seems therefore to depend rather on the insensibility of the muscles; and this experiment consequently appears to show that the movements of the muscles, whose sensibility is removed, become un-co-ordinated, because the brain does not acquire the impression of, and the animal therefore does not become conscious of, what takes place in them, just as it seems also to follow that the will cannot dispense with the aid of the sensitive nerves in the production of voluntary movements. Completely corresponding results are obtained too in the mammalia. If, for example, we divide, in a dog, the posterior roots of a limb, the animal, when standing, holds the latter half bent, raised a little above the ground, and

when walking or running he drags it after him; if he attempts to stand upon the hind legs he falls. If he lies, he keeps this leg constantly extended; voluntary movements he does not execute with it, while on the other hand, it continues to be the seat of aimless and un-co-ordinated, though powerful movements. But if only one posterior root be left untouched, so as to leave some sensibility in the leg, its movements retain also their regularity, and none of the phenomena just described occur. If, finally, only the cutaneous nerves be divided, even in all the four limbs, there is no impediment to walking or running, and the movements take place regularly, probably because the muscular sensibility is retained. Division of the posterior sensitive roots thus produces in the higher animals also, exclusion of the voluntary movements; in them, too, therefore, sensibility and the will are physiologically mutually connected. That this obtains also in man, not only analogy, but also daily experience testifies. If, for example, we wish to lift a weight, we first make weak experimental contractions to inform the muscular sense of the strength of the necessary effort; the information, which thus precedes the final voluntary muscular contractions, is, of course, due to the sensitive nerves.

If it be now shown by these experiments that sensation and motion stand in a much more intimate relation to one another than had before been supposed, it is also easy to understand that results similar to those produced by division of the sensitive nerves may be developed by their atrophy or degeneration. In the latter case we speak, of course, only of a certain degree of change; by division of a nerve its function is necessarily destroyed, every trace of conduction through it ceases; in more or less complete degeneration conduction is weakened or ceases in a given number of its fibres, while it still takes place through the non-affected fibres. The parts of the body to which the nerve is distributed will therefore retain some of their sensibility, but their movements will, in a higher or lower degree by their inco-ordination, present evidence of the corresponding lesion. The integrity of the anterior motor roots causes only that the strength of the movements, the degree of contraction, and the energy of each particular muscle are not diminished, but cannot prevent this result with respect to the complex movements.

The information which we can derive, respecting the physiological basis of the disease, by examining the condition of the spinal cord, s indeed also important; though, on the whole, it is of less value, because the physiological functions of the several parts of the cord cannot yet be said to stand upon so sure a basis as those of the nerves. Many circumstances connected therewith still require to be investigated, and it is probable that posterity will decide many questions in this department otherwise than we have done. Nevertheless we shall see that our present knowledge of ataxy is capable of, to a certain extent, satisfying our requirements, and of explaining the mechanism of the disease.

With respect to the properties of the spinal cord, it is now proved, contrary to Bell's assumption, that the two substances have in it totally opposite relations to what they present in the brain. While in the brain the peripheral grey substance is insensible and the central white matter is sensitive, in the spinal cord, on the contrary, the peripheral white substance is everywhere sensitive, while the central grey substance is throughout insensible. All the three columns of the spinal cord are therefore sensitive; the animal perceives and reacts against any irritation of them; but still there is the essential difference between the sensibility of the three columns, that that of the anterior and of a part of the lateral columns is only recurrent, and is communicated to them through the anterior nerve-roots, while that of the posterior, and perhaps of a part of the lateral columns, is independent. If the anterior column be pricked, the animal perceives and reacts against it only when the part touched is in normal connexion with the nerves arising from it; if, in fact, the corresponding anterior root be divided, this part and the adhering central extremity of the nerve become forthwith insensible, while the peripheral end of the nerve still retains its sensibility; and if the corresponding posterior root be divided, without the anterior being touched, not only the whole of the anterior root, but, at the same time, its place of origin in the anterior column of the spinal cord, loses its sensibility. The sensibility of the posterior column is, on the contrary, not at all affected thereby; it continues, especially towards the median fissure, even if all the posterior nerve-roots be divided. This shows clearly that it is only the posterior columns which are the source and seat of the sensibility, and that it is from them that the latter proceeds to all parts of the body; it proves, at the same time, that the sensibility of the anterior and lateral columns is not communicated to them directly from the posterior columns, but only by a circuitous route, inasmuch as it first passes out through the posterior roots, and then back through the corresponding anterior roots. If all the anterior roots were divided, the anterior columns would, therefore, be quite insensible throughout their whole length; if all the posterior roots were divided the posterior columns would still, on the contrary, everywhere have retained their sensibility. The grey central substance is invariably insensitive. If the spinal cord be divided, and the grey substance be irritated in one of the surfaces of section, the animal does not observe it, and does not react against it.

More important still are the recent discoveries respecting the functions of the several parts of the spinal cord, which are now considered to be quite different from what they were believed to be in Bell's time. If his views, described above, were correct, on division of one or more of the columns, the nervous manifestations which he believed to be conducted through them should altogether cease in the parts of the body situated beneath the section. This is, however, far from being the case. The

experiments of Stilling, van Deen, Türck, Brown-Séquard, and many others, have on this point elicited results which quite corroborate one another, and which lead to conclusions totally different from those of Bell. Thus, if in a frog one half of the spinal cord be divided, the subjacent parts of the corresponding half of the body lose neither their sensibility nor their motility; on the contrary, both functions are enhanced thereby. If, as Türck first observed, both hind legs of a frog so operated on be dipped into a very weak solution of sulphuric acid, we shall find that the leg on the side operated on is drawn more rapidly back from the fluid than the sound leg; that it, therefore, feels more acutely, is hyperesthetic. The retraction of the leg shows that neither has motion been paralysed, as the reflex action is produced; nay, if we look more closely we shall, especially if some time has elapsed since the operation, even find that all the movements of this leg are more energetic, that the contraction of its muscles is, therefore, stronger than that of the sound leg. Hence, quite contrary to expectation, both sensibility and motility increase in the subjacent parts, and this not only in the superficial tissue, the skin, but in all the tissues; even the proper sensibility of the nervous tissue, especially the recurrent sensibility of the anterior roots becomes, as Bernard has shown, below the section greater on the side operated on than on the sound side. The same takes place when the section is complete; if we cut the spinal cord across we shall see that the frog so operated on draws both his hind legs back from the surface of the acidified water more rapidly and with greater force than a sound frog does. Nevertheless there is, of course, this great difference between the effect of these two operations, that only in the first case can the sensation be conducted through the uninjured half of the spinal cord upwards to the brain, and be perceived; in the second case, on the contrary, it must necessarily remain unperceived. With respect to the frog this conclusion is arrived at only *a priori;* in the case of the higher animals it can, on the other hand, be directly demonstrated. Thus, if we perform the same experiment on a rabbit, we obtain precisely the same results, both sensibility and motility being increased. But it is only in the case of the incomplete, unilateral section of the spinal cord that the retraction of the leg is accompanied by a cry, evidence that the animal feels and is conscious of pain. When the section is complete there is no cry; the retraction of the leg is a reflex phenomenon wholly without consciousness. If we now wish to know through which of the columns remaining in the uninjured half of the spinal cord sensation is in the first case conducted to the brain, this may be ascertained by way of exclusion. We then find, in fact, that we may, on this hitherto sound side, not only divide the anterior and lateral columns without preventing the occurrence of a cry, the evidence of the conscious reception of the sensitive impression, but that we may even divide the source of the sensibility itself—the posterior column;

nay, that we may go so far as to remove a portion of it. There consequently remains only the grey substance, through which the impression can and must be conducted to the brain; and the result of the whole of this series of experiments, then, is, that the sensibility, which proceeds exclusively from the posterior columns, is conducted, not through them, but only through the in itself insensible, grey, central substance, to the seat of consciousness. The fibres of the posterior roots may, therefore, in part pass over into the posterior columns, but they must at the same time pass, in an essential degree, into the cells of the grey substance, and in this transition lose their original sensitive property. Whether some of them, as many writers—for example, Türck—assume, finally pass over into the lateral columns, is not so certain. The conduction of the sensitive impression to the brain seems, at all events, to take place only through the grey substance.

With respect to the motility, we have already seen above that many arguments may be adduced in favour of the view that the conduction from the brain, which is connected with the voluntary movements, takes place through one or other of the organs of sensation. As to the course of the involuntary reflex movements, our knowledge is more certain, for we know that in their occurrence three stages are to be distinguished :— 1, the sensory impression; 2, its transformation into motor excitation; and 3, the movement—that their nervous course consequently proceeds from the sensory nerve, passes through the spinal cord, and ends in the motor nerve. Now, in what part of the spinal cord does the transition from one kind of nerve to the other take place? Most certainly in the grey substance. The sensory nerve filaments pass, in fact, into its posterior part; and we know that from the cells in its anterior horns not only the motor nerve-filaments take their origin, but also that numerous connexions proceed in all directions, not merely to the parts lying above, below, or on the same level in the opposite half of the cord, but likewise to the posterior horns. Each of these cells thus becomes a small nervous centre; and it is, therefore, quite in accordance with this fact that by touching some points of the spinal cord we produce whole groups of movements.

The grey substance must, accordingly, be considered not only as a conductor of sensation, but also as a centre of reflex movements. Where the conduction of the voluntary movements takes place, we, on the contrary, do not yet know with certainty. What, on the other hand, we do at present know with respect to their occurrence concerning the spinal cord is, that the posterior columns have the same importance for them as the posterior roots. Thus, while we have seen that the motility is in a certain manner increased after a simple division of them, a totally different result is obtained when, as Philipeaux and Vulpian were the first to try, the posterior columns of the spinal cord of a dog are divided in two

places, from one to three inches from each other. According to their experiments the sensibility of the hind legs is thus increased, but their motility is much weakened; the animal can no longer stand erect upon its hind legs, and walking is accomplished essentially only with the fore legs, the hinder limbs being dragged after; it is a true traumatic motor paraplegia which is thus produced. Pathological experience confirms these physiological results. In two cases—the one described by Cruveilhier, the other by Stanley—where the posterior columns of the spinal cord were the only parts affected, and in which especially all the nerve-roots were perfectly normal, the sensibility was unimpaired, but the patients could neither stand nor walk, and in the sitting position could only with great effort raise their feet from the ground. Independently of the fibres running in them from the posterior nerve-roots, the nerve-tissue itself of the posterior columns has, therefore, a great influence on the more complex movements, especially on those required for walking or standing. It seems as if the function of these columns is to combine the peculiar actions of the several parts of the spinal cord, and duly to co-ordinate the movements of the individual muscles. If the posterior roots, or merely the fibres passing from them through the posterior columns, are at the same time altered, both sensibility and motility must be blunted, provided that the lesion does not occupy too small an extent; for if its extent be small—if, for example, it occupies only the space of one or two vertebræ—it may, as pathological experience has shown, exist in man without being recognisable by any symptom.

Lastly, we must remember that sensibility presents itself under many different forms, of which the most important are—1, the general sensation of contact; 2, the sensation of pain; 3, the sensation of temperature; and 4, the perception of muscular activity. That each of these forms may by itself be altered without the others suffering thereby, or, even if they are affected, suffering in the same manner; and, lastly, that one and the same influence, but of different strength, or one and the same organic lesion, but at different periods, may gradually induce their exaltation, perversion, and diminution, or extinction. In these respects the experiments with compression of the nerves, instituted by Bastien and Vulpian, are especially interesting. Thus if we compress one or other nerve—for example, the ischiatic nerve—the general cutaneous sensibility is soon blunted; but if we at this moment prick or pinch the skin, or touch it with a cold body, we cause very violent pain; and it is only upon stronger compression that this hyperesthesia is replaced by anesthesia. The same holds good with respect to the spinal cord; pathological observations as to the effect of compressing exudations or of tumours sufficiently establish this point. But if this be the case, it is not difficult to understand that the disease under our consideration, which is due precisely to congestion of the posterior columns and posterior nerve-roots of the

spinal cord, and to the consequences of the same, may be attended with excessive pain.

We shall now, in conclusion, find that we shall be able, at least in a certain degree, to refer the symptoms of ataxy to this physiological basis. The cause of the affections of the eyes has already been stated, and is sufficiently evident; the pains are explained, as has just now been mentioned, by the congestive process and its results; the disturbances of mobility and of sensation find their adequate cause in the atrophy and degeneration of the posterior columns and roots, and in the close connexion which modern researches have proved to exist between sensation and motion; and lastly, the abnormities in the urino-genital system depend upon the numerous anastomoses of the cerebro-spinal nerves with the sympathetic. The symptom which must still be looked upon as most enigmatical, is properly the state of the sensibility. Sensation certainly would necessarily be wholly extinguished so soon as the posterior roots were entirely destroyed, and accordingly we have, undoubtedly, in the fact that in none of the dissections on record such complete destruction was met with, but that some nerve fibres were still always preserved—an explanation of the circumstance, that in most cases the sensibility was only more or less weakened, and not entirely removed, and especially that it was not equally affected in its different manifestations; but beside these cases there are others, as I have already several times stated, fewer in number, but entirely trustworthy, where the sensibility was not found to be in any way affected, and where, nevertheless, the disorganization of the posterior roots was very far advanced. Are the nerve fibres still remaining sound which are met with in the midst of the destroyed ones, sufficient to maintain the function of the nerve without any discoverable change? Or are we, as, for example, Charcot and Vulpian do, justified, in the face of so striking an opposition between the results of clinical experience and some of the most certain conclusions of experimental physiology, in consigning the former to oblivion and in inferring that some mistake or other lies at the bottom of the statements of the several observers with regard to the normal existence of sensibility in each respect? Are we, for example, warranted in believing, as Jaccoud first pointed out, that the muscular sensation, like that of the skin, has different manifestations—that each of these may be separately affected, and that we therefore ought not to assume it to be decidedly normal, because the patient, for example, has the sensation of contraction of the muscles, and feels pain in them when they are pressed upon, or when they are the seat of convulsive spasms? This question we must leave unanswered, there is a contradiction involved in the point which we are not yet in a position to get rid of. The same is the case when the state of the sensibility is considered from quite another aspect. Thus, if we reflect, that more or less of the fibres of the nerve-roots are destroyed, we should,

in the patients in whom a greater or less degree of anesthesia of the skin exists, expect that this would be of such a nature that parts would be found with healthy cutaneous sensibility scattered among others completely anesthetic. This, however, is never the case; the sensation is uniformly weakened over the whole of the affected surface of the skin; it is no where normal, but neither is it anywhere entirely extinguished. This phenomenon cannot be explained in any other mode than by taking refuge in the view adopted by most modern physiologists, that the sensibility, even in the state of health, radiates, that is, that it is found not exclusively where distinct nerve-fibres can be demonstrated, but that it has a greater extent. Its existence is therefore not, in the strictest sense, connected with the immediate presence of nerves; the space between two nerve-fibres without lateral branches will, in fact, on this assumption, also be sensitive. Lastly, neither does our present knowledge teach us what the reason is that sensation in general, even in extensive ataxic lesions, is not more affected than is the case; while, on the contrary, we see that, in other apparently less intense lesions, both of the spinal cord, and of the brain, for example, in apoplexy, all sensation, both direct and indirect, is completely and permanently lost.

Another circumstance which, in like manner, cannot yet be explained, is the connexion which in this disease exists between the spinal lesion and the affections of the optic nerves without the intervening nervous parts being in any way implicated. We have no indication on which to base any opinion as to the cause of this fact, and yet it is not possible to consider the abnormities as independent of each other and only accidentally co-incident; we are, on the contrary, compelled to regard them as two connected joints in the chain, between which the future may reveal the uniting link.

One short remark still remains to be made before closing this article. The question must, in fact, suggest itself to every one, whether the motor disturbances are merely the result of the weakened sensibility, or whether they exist more independently. From all that has gone before we must adopt the latter view. There can, of course, be no doubt that the impairment of sensation, whether it occurs in the skin or in the muscles, or in both, must contribute considerably to increase them, especially when vision is at the same time affected; but the cases in which no anomaly of sensibility is demonstrable are decidedly against assuming its disturbance to be the primary affection. Before these cases were observed, the decision was not so easy; both opinions had their advocates. Thus Duchenne shortly after his first description of the disease as a new morbid affection, was violently opposed by Landry, who had in 1855 communicated a series of observations respecting patients with paralysis of the muscular sensibility, and now asserted that ataxy was only another name for this disease, that the priority of this discovery belonged to him,

and that Duchenne had consequently been guilty of a literary mistake. Each party soon acquired adherents; Trousseau took Duchenne's side, and asserted that in ataxy, besides the symptoms met with in the paralysis described by Landry there are others, and that the differential diagnosis is therefore possible between these two diseases; Landry's opinion, on the other hand, was supported by Becquerel, Monneret, Marcé, and others. After the publication of Bourdon's and other similar observations this contest was given up; it appears to me, too, that the question must be looked upon as having been thereby decided. On this point Duchenne has had the right on his side.

CLINICAL RECORDS.

Cases Illustrative of the Loss of the Proper Use of Letters, and of Words, as a Result of Cerebral Disease. By William Daniel Moore, M.D., Dub., M.R.I.A., Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Examiner in Materia Medica and Medical Jurisprudence in the Queen's University in Ireland.

The following cases I should gladly have offered to my friend, Professor Banks, as a contribution towards his important memoir "On the Loss of Language in Cerebral Disease," which appeared in the 77th number of this Journal (for February, 1865), had I been aware that such a paper was in preparation by him. As I lost the opportunity of doing so, I would now place them on record, as an appendix of illustrative cases to his interesting essay. In doing so, I regret very much that my notes of the first case I shall mention are very imperfect; and that of the second case, which occurred to me some years previously, I have no written record whatever. Nevertheless, as the particular point to which I wish to call attention—the loss of the proper use of letters and of words, as a result of cerebral disease—is in one instance noted, and in the other is, by a couple of examples, indelibly impressed upon my memory, I trust I may be allowed, even now, to place upon record the instances to which I have alluded, as illustrations of the ever-varying phases of the symptoms of brain disease.

Case I.—On the morning of Sunday, December 10th, 1854, I was called to see E. H., Esq., aged fifty-six, who had all his life enjoyed good health, except that latterly he had been annoyed with dyspeptic

symptoms, and especially with uneasiness in the left hypochondrium. I found him, at six o'clock, a.m., apparently suffering from colic, which had set in about five hours previously, and for which he had taken castor oil, and attempted to administer to himself an injection. A large emollient enema, thrown up with the long tube, brought away a copious accumulation of fecal matter mixed with scybala. This relieved the patient for a short time, but in the course of an hour or two he was seized with intense pain and tenderness in the left hypochondrium, accompanied with much uneasiness in the entire of the transverse arch of the colon; his tongue was dry, and brownish in the centre. Sir Henry Marsh then saw him with me. Two relays of eleven leeches each were applied to the seat of tenderness, and calomel and opium were freely given; this treatment was attended with much relief; poppy-stupes also gave great ease. On the 12th the patient referred the pain more to his back, and he once complained of uneasiness in one testicle. Mr. H. progressed favourably until the 16th (his pulse had never risen beyond 92), when, with great languor and lassitude, he passed a quantity of bloody urine, acid, s.g. 1·016, free from sugar, containing a fair amount of urea, depositing much mucus, numerous blood-corpuscles, some epithelium, and tubular casts. Next day the blood was much less in quantity; on the 18th it had ceased to be perceptible to the naked eye, but blood-corpuscles were still visible in considerable number under the microscope. The urine now also deposited numerous large crystals of uric acid, many of them agglomerated, and some arranged in long series, having apparently formed on hairs. 19th.—Mr. H. seemed low and weak, but was otherwise going on well. It was a question whether the pain Mr. H. suffered from, as well as the hematuria, might not have proceeded from the presence of a renal calculus. Some apprehension of incipient Bright's disease was also entertained.

In October, 1858, Mr. H., who was then at his residence in the west of Ireland, on returning from church, fell asleep, and awoke hemiplegic. He was largely bled and actively treated. The loss of power soon passed off, but his sight continued impaired; and his memory, especially for names of places and persons, was very much affected.

Nov. 11.—To-day I observed that when Mr. H. (who had come up to Dublin for further advice) closed his left or sound eye, he could not open the right or affected eye. He appeared not to see with the latter, for when it was *held* open, the left eye being shut, the right eye did not take the direction of the object at which the patient was desired to look. When the right eye was closed, the left remained open, and Mr. H. could see. When both eyes were open, no peculiarity was observed, but the inability to raise the lid of the right eye when the left was shut, and *then only*, was remarkable.

November 18, 1858. — The above-described phenomenon was not

permanent—neither Sir Henry Marsh nor I succeeded in observing it two or three days later. But this day I found that, though Mr. H. gives right names to persons and things, and speaks correctly—except that he is sometimes at a loss for a word—he completely miscalls letters, calling A K, &c.

Feb. 16, 1859.—Mr. H.'s general state is one of improvement; he has been out several times, *but, notwithstanding that he is able to draw and sign a cheque correctly, he still miscalls letters, and consequently cannot read a word.* He complains of shooting pains in his right side, and though there is no apparent loss of power, that side is evidently wasting. As to letters, a slight improvement was apparent the other day, when he spelled and read correctly "the ne," though he miscalled the next letter, which was "w."

Mr. H. died of a sudden attack of apoplexy October 1, 1860. He had not experienced much improvement since the last report. He was seen by Sir Henry Marsh, who that day two months, December 1, 1860, himself died suddenly of a similar attack.

Case II.—On Friday, the 4th September, 1840, R. H. G., Esq., aged about fifty-six, was attacked, at his seat in the County Wicklow, with paralysis of one side of his face. Being alarmed, he came into Dublin, and in the course of the day he became completely hemiplegic. Sir Philip Crampton and Dr. Stokes both saw him with me. On Thursday, the 10th, he was much better; and, sitting up in his room, said to me—"I will go to bed to-morrow." This expression he repeated several times; he meant that he would go home the next day. On the following day, however, he got a much worse attack of hemiplegia, and now the loss of connexion between ideas and words was complete. On one occasion I was much puzzled by his saying to me repeatedly, "Clean my boots." Finding that he was not understood, he became much excited, and cried out vehemently—"Clean my boots by walking on them!" At length it was ascertained that the cause of his disquietude was the shining of the candle on his face, and that the object of his unintelligible sentences was to have the curtain drawn; when this was done he appeared quite gratified. Mr. G. slowly improved from this second atack, but became a lunatic, in which state he survived for fifteen or sixteen years.

In the latter case it is remarkable, that although, after the second attack, there was no connexion whatever between the words used by the patient and the ideas intended to be conveyed, Mr. G. formed complete sentences.

Cases treated at the Dispensary for Skin Diseases, Bishop-street. By T. W. Belcher, M.D. Dubl.: Fell. R. Coll. Phys. &c. (No. III.)

In the February and May numbers of this Journal I gave particulars of several cases illustrative of important points in practice. I now

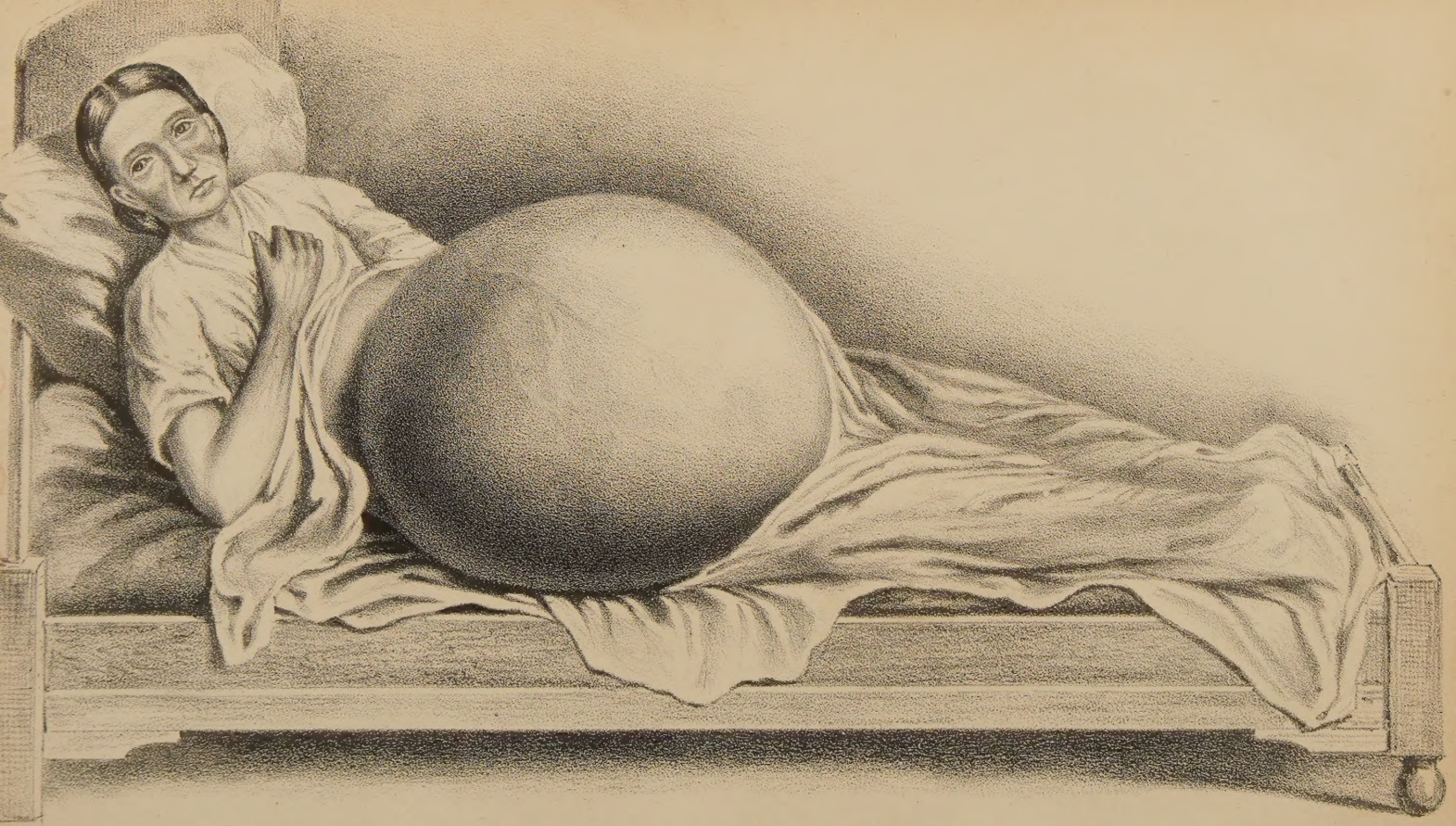
venture to give one illustrative of the great difficulty of diagnosis, and classification, in many examples of cutaneous disease, as well as the variety of treatment, and the amount of patience often necessary to effect a cure.

Case XI.—S. P., aged fifty-six, a tradesman's wife, who was in the habit of washing clothes a good deal, was admitted 7th October, 1864, with an eruption on her hands and arms, and a slight erythema on one cheek. She has had this affection to a greater or less degree for some years, and has tried many remedies under the advice of various physicians, with none of whom she had attended sufficiently long to get any good. The eruption is symmetrical on both forearms and hands; in shape nearly circular, attended with intense heat and itching, loss of appetite, and general *malaise*. The disease is in three different stages: 1, on the right arm, a vesicular eruption, with what is commonly known as an eczematous discharge; 2, on the left arm, an impetiginous eruption, with purulent discharge; and 3, on the left cheek, a circular erythematous blush. According to Neligan's classification, the right arm would be said to be affected with eczema impetiginodes; the left with impetigo figurata; and the face with erythema simplex. To this I may add, that in about a fortnight after admission the diseased skin of the left arm became *rimous*, answering to the "eczema rimosum" of Dr. M'Call Anderson (*Practical Treatise upon Eczema*, p. 23).

In the first instance, she got three minims of liquor arsenicalis, thrice daily, after meals; and she was directed to keep the affected parts covered with mucilage of starch, to be changed daily. Her system being soon under the influence of arsenic, its use was suspended, and quinine substituted for it. After a few days the use of the former mixture was resumed, with directions to take a small quantity of sulphate of magnesia twice weekly. Although the disease appeared to get worse shortly after beginning the use of arsenic, in about twenty-one days she seemed to get very much better. The arsenic disagreeing with her a second time, quinine was again substituted for it; and the mucilage of starch failing to do any good, she applied to the skin linseed poultices moistened with liquor plumbi dilutus, and used the following ointment:—℞ Ferri sulph. siccat. gr. iii.—Chloroformi M. v. Ung. simpl. ʒi. M. This relieved the local symptoms very much, and the chloroform was afterwards increased or lessened in quantity as occasion required. Again she took to the arsenic, and now with better effect. Since then she was able to take it for some time, until the peculiar arsenical desquamation, described by Hunt, appeared. She was again compelled to stop the arsenic, and take to the quinine; the eruption, however, gets better, and the itching and exudation alternate from one arm to the other. About two months after admission she commenced taking quinine in combination with arsenic. This served

her very much. She also applied Hebra's tincture (described in the May number of this Journal, p. 496), to the skin, but this did not answer, so tincture of iodine, prepared as described in the February number of this Journal (p. 253), was applied to the surface for a considerable time with good result. Chloroform was now the only external application which relieved the itching; and it was freely used with this view. The eruption gradually narrowed in its circumference; the nature of the discharge frequently alternated; now the disease seemed almost gone, and again it appeared to gain fresh vigour. At last it entirely disappeared; and after she had taken the combination of quinine and arsenic for some time subsequent to the apparent cure, she was discharged on the 19th of May, 1865.

If I understand Mr. Erasmus Wilson aright, it would seem that this case is what Willan would have called "washerwoman's itch;" and what he (Mr. E. W.) calls "eczema dorsi manus," "commonly termed psoriasis" (*Student's Book of Cutaneous Medicine*, 1864, p. 87). The case just detailed shows how necessary patience and variety of treatment is in many instances of cutaneous disease; while I confess that I cannot properly classify it according to any system, unless I accept Dr. M'Call Anderson's statement, "that cases of eczema are often met with in which an erythematous state of the skin, vesicles, pustules, papules, and fissures are met with in a combined form."—(*Op. cit.* p. 3). This is one of several doctrines held by him in connexion with Hebra's theory—that eczema is a disease of wide comprehension, and of great variety; and that (contrary to the dogma of Willan) it is not exclusively vesicular.



Mr Butcher on Ovariotomy and the after treatment of the patient.